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ON HEREDITY AND REJUVENATION.

BY CHARLES SEDGWICK MINOT.

(*Continued from page* 9.)

III. A COMPARISON OF LARVA AND EMBRYO.[8]

It has long been known that animals develop according to two types, appearing in their younger stages, either as larvæ or as embryos. The larvæ lead a free life and must obtain their own food. Embryos, on the contrary, do not lead a free life and are nourished by the yolk accumulated in the parent ovum. There is, of course, no absolute demarcation between the two classes; nevertheless, a general comparison between them establishes several conclusions which throw valuable light upon some recent biological hypothesis.

First of all, it must be remarked that the larval development is primitive, and that the embryonic development has been evolved later. Geologists are able to present two principal supports for this assertion: 1. In the lower animals we encounter only larvæ, never embryos; sponges, colenterates, echinoderms and worms, all pass through the early stages of

[8] Read before the Amer. Soc. of Morphologists, December, 1893.

their ontogeny as larvæ. It would, therefore, be superfluous to linger for the defense of a view which is already accepted by all biologists. 2. The embryonic development depends on the presence of yolk. Now we have learned that the yolk has developed very gradually and in all the lower animals appears only in small quantities. It was not until the increase of yolk material had become enormous, as, for example, in the meroblastic vertebrates, that we find the development completely embryonic in type. With the increase of the yolk comes the gradual transition from larval to embryonic development. Since the embryo is dependent on the yolk, and since the yolk exists only in the higher forms in sufficient quantity, it follows that fully typical embryos can occur exclusively in the higher (later developed) animal types.

The fact that larvæ represent the primitive forms of development, obliges us to conclude that the correctnesss of Weismann's theory of the continuity of germ plasm can be tested better in larvæ than in embryos, since in embryos the relations have undergone profound modifications by secondary changes, which in this connection might easily deceive us.

I do not venture to assert that I know what the present form of Weismann's continuity theory may be; I hold, however, the exact form of this much discussed theory to be non-essential, because, according to my conviction, the theory can in no form be brought into agreement with our present knowledge. Nussbaum founded the theory, and opened the way along which we certainly hope to make great advance. Let me acknowledge the great value and the strictly scientific character of Nussbaum's work; doing this not merely because I esteem it, but also because the unjust attempt has been made to diminish his claim. Nussbaum[9] thought that the germ cells are *direct* decendents of the fertilized ovum, keeping the germinating power. while the rest of the cells developed from the egg are transformed into the tissue of the body. He brought forward several facts which could be interpreted in favor of his theory. By this theory the whole problem of her-

[9] M. Nussbaum, Zur Differenzierung des Geschlechts im Tirreich, Arch. f. Mikrosk. Anatomie, XVIII, 1--121, (1880).

edity and development was stated in an entirely new form. Since this publication of Nussbaum's we are seeking for the explanation of the germinating power, and the propagation of this power; formerly we sought for the causes of the inheritance of parental parts. The difference may be illustrated by the following example. Before Nussbaum we were ruled by Darwin's conception of Pangenesis, and we investigated accordingly for the agency by which the eye of the father reproduced itself in the child. Since Nussbaum we leave Pangenesis behind—it belongs henceforth to the past—and try to determine how the germinal substance behaves, and especially in what way it is perpetuated from the ovum through the following developmental stages, so that it is finally still present for the creation of the next generation. It is the conception of the continuity of the germinal substance which we prize so highly, and owe to Nussbaum.

Larvæ teach us that it cannot be special cells which affect this continuity. In fact, we find the organs of larval life fully differentiated before any sexual organs are recognizable, and indeed, in the majority of known larvæ we cannot recognize even the rudiments of the sexual glands. On the contrary, we find in larvæ unmistakably differentiated locomotive apparatus, such as cilia and often muscle fibres, a digestive canal, sensory organs, and, in many cases, also special excretory organs, and yet, only in a very few and exceptional cases can we distinguish the cells which belong to the future sexual glands. Thus, in regard to the primitive or larval type of development, we cannot say that the germ cells are constantly separated from the somatic cells during the segmentation of the ovum, but must rather draw precisely the opposite conclusion, namely, that the germ cells belong to the tissues which arise latest. We often meet many tissues in larvæ at a time when there is still no indication of germ cells. We find the same relations in embryos also, since in them the principal tissues become recognizable before germ cells are present. This fact was well established for vertebrates many years ago. It is characteristic of Weismann that he long defended the continuity of germ cells, in defiance of the facts. He has since

given up this wrong view and put in its place his hypothesis of the continuity of germ plasm. Of Nussbaum's conceptions, Weismann has left out the fruitful part, and has sown broadcast those ideas which were incapable of fruitful development. He has attempted to defend his notion of the difference between the elements of the embryo destined for the construction of the body, on the one hand, and those elements destined for sexual propagation on the other. Now, since the sexual cells usually develop from somatic cells, he was forced to assume that there is a mysterious substance which he names "*Keimplasma.*" This substance is supposed to store itself in the body by some secret way, to separate itself at command from the histogenic plasm, to appear unchanged and ready to be the exclusive agent of hereditary transmission.

Nussbaum furnished the conception of the continuity of germinal substance, which appears to be of immeasurable importance for the scientific investigation of the phenomena of heredity. But this continuity holds for all cells which arise from the fertilized ovum, as explained in the first section of this article. We must, therefore, seek for the causes of the differentiation of cells, that is to say, for the causes of the production of nerve cells, muscle cells, gland cells, etc, *and* of the production of germ cells.

I will now try to make clear the significance of the comparison between larvæ and embryos for the interpretation of germ cells. This calls for a short digression.

In the course of my investigations on "Senescence and Rejuvenation," of which only the first part has been published (Journal of Physiology, xii, 97), I learned that as cells become older there occurs an increase of the protoplasm in proportion to the nucleus, and I further succeeded in proving, as an essential process in reproduction, the formation of cells with comparatively little protoplasm. Further, it was found probable that a rapid multiplication of cells is only then possible when the cells have small protoplasmatic bodies (Proc. A. A. A. S., XXXIX (1890). We, therefore, have learned that the power of development depends on a special condition of the cell. By these facts I have been led directly to the following hypothesis:

The development of an organism does not depend on a substance stored in special cells, but on a special condition (stage) of organization. As a corollary of this hypothesis may be given this conclusion: *Germ plasm, in Weismann's sense, does not exist.*

According to my view, every part inherits from the germ, and every part of the animal body, as well as its germ cells, possesses the multiplying morphogenetic force, the action of which, however, is inhibited to the condition of the parts themselves. What this condition may be is not yet exactly known, but this much we do know, that the morphogenetic force is found in full activity in cells with little protoplasm. It is indeed highly probable that the slight development of protoplasm in proportion to the nucleus is an unavoidable condition of morphogenesis, or in other words, of the action of heredity. In fact we see that the first processes of development —as I have elsewhere explained (Proc. A. A. A. S., XIX)— show in the most varied cases a remarkable uniformity, for they always accomplish the production of cells with little protoplasm. Compare, in this respect, the vegetation points of plants, the root buds of slips, the budding zones of Annelids, the germinal layers of vertebrates, etc. The condition which allows the morphogenetic or hereditary force to act, arises under differing conditions, of which the fertilization of the ovum is one only.

Weismann tries to make comprehensible to us this one case, that of the fertilized ovum, by a special explanation which is available for no other case. Oscar Hertwig has recently (Zeit und Streitfragen, Heft I) clearly shown that Weismann's explanation is a speculative assumption, which can only be saved from rejection by numerous and often selfcontradictory additional assumptions. As I fully agree with Hertwig's criticism, I need only refer to his essay.

We will return to our proper theme. The next point is to determine whether there is a difference in the condition of the cells, as, regards their capacity for development, between larvæ, on the one hand, and embryos on the other. It can be proved that this is the case, by the following considerations. So far as we yet know, it is chiefly two factors which inhibit

development: *first*, the increase of protoplasm; *second*, the progress of organization, i. e., of differentiation.

As I was about to close this article, I received through the kindness of the author, Nussbaum's address on differation, in which he has defended essentially the same views as those which I hold. Such an agreement is of great value to me.

Now we know that larvæ are animal forms which have to obtain their own food and to protect themselves against enemies, and therefore are provided with differentiated tissues. Embryos, on the contrary, take their nutriment simply from the ovum, and the cells continue for a long time, developing and multiplying, while the protoplasm of the single cells increases very slightly, and *the beginning of the differentiation proper is correspondingly postponed.* I believe that we here have to deal with causal relations. From the actual relations just described, I conclude that the most essential difference hitherto known between larvæ and embryos, is to be found in the differing lengths of the period of multiplication of undifferentiated cells. In consequence of the shorter duration of the period in larvæ they have a much smaller total number of undifferentiated cells than embryos, or reversely expressed, embryos are much better equipped with material for the construction of the adult body, than are larvæ. As already stated, embryos are produced by the higher animals. This fact finds its explanation in the relations just described, because the increased number of undifferentiated, or so called embryonic cells, is precisely the necessary preliminary condition of the greater complexity of the differentiation by which the animal becomes more highly organized.

For the sake of clearness I have put aside all complications which might come in to play. It goes without saying, that the relations, in many respects, are by no means simple, nevertheless, the main conclusion above given seems a secure gain.

I therefore interpret the embryo as a device to render possible the increase of undifferentiated cells, and consequently a higher ultimate organization. The origin of this device is conditioned by a supply of food independent of the embryo.

From our present standpoint it is a matter of indifference whether the independent food supply comes from the yolk or from the uterus, however important the difference may be from other points of view.

It is to be further noted that our interpretation of the significance of the embryo is also opposed to Weismann's theory of germ plasm, because it emphasizes the importance of the *condition* as opposed to the assumption of a germinal substance or plasm. This road also leads to the conclusion reached above by other ways, the conclusion, namely: Reproduction involves rejuvenation, and rejuvenation is characterized by the production of cells with little, and that little not differentiated, protoplasm. Since rejuvenated cells arise by asexual as well as by sexual reproduction, since they appear in much greater numbers in embryos than in larvæ, and since they may be interpolated, as in the pupæ of butterflies, in the midst of the development of an individual, we must admit that the hereditary impulse (*vererbende Kraft*) is distributed in very different cells and is probably distributed equally through all cells. Hertwig has reached the same conclusion, with which Weismann's theory of germ plasm cannot be made to agree.

As Weismann has neglected the problem of rejuvenation, he has necessarily often gone astray in his discussion of phenomena in which rejuvenation plays the principal role. One is astonished at the slight attention bestowed on rejuvenation, when one recalls that it is the central problem of all questions of heredity treated by him.

Rejuvenation is one of the principal phenomena of life, and the rejuvenated condition of the cell is probably an unavoidable preliminary of heredity. We know that at least one *anatomical* sign of the rejuvenated condition is to be found in the preponderance of the nucleus in proportion to the protoplasm: a second *anatomical* sign is found in the structure of the protoplasm, which, in young cells always remains without differerentiation. The chief *physiological* sign of rejuvenation in cells which we as yet know is the power of rapid multiplication. Thus, we see, in case of sexual rejuvenation, that the

development of the fertilized ovum begins with an excessive proliferation of the nuclei, by which numerous cells are created, each with little protoplasm. Histogenetic differentiation begins later. The asexual rejuvenation has a similar course, but needs more thorough investigation.

Now differentiation is the sign of inheritance, and this morphological inheritance cannot develop itself fully until the senescence of the cells becomes recognizable by the growth of their protoplasm. On the other hand, we see complete inheritance develop itself, after preceeding rejuvenation. Accordingly we gain two conceptions: *first*, the hereditary impulse belongs to the inherent and constant properties of cells in general; *second*, the activity of their impulse may be inhibited by the condition of the cells. My view may be expressed in the following way: Somatic cells are simply cells in which the activity of the hereditary impulse is inhibited in consequence of their senescence, or, in other words, differentiation; but under suitable conditions the somatic cells may pass over into the rejuvenated stage, and thereupon develop the most complete hereditary possibilities.

The importance of rejuvenation must also be recognized when we consider the phylogenetic origin of single organs. Let us take a simple example. We may safely assume that the ancestors of mammals possessed a smooth skin, and that the covering of hairs is a new acquisition. Each hair is the product of a local growth. If we investigate the germ of a hair, we find that it consists of rejuvenated cells, that is to say, of cells with little protoplasm, or, as we are accustomed to say, of the embryonic type. Thus the formation of hairs depends on numerous centers of rejuvenation. In the multiplication of striped muscle fibres we find the agents to be the muscle buds, which are small, protoplasmatic structures, with relatively numerous nuclei. If we observe a developing gland, let us say a pancreas or a sweat gland, we find the rudiment to consist of rejuvenated cells; the cells multiply rapidly, and after the organ has its essential form, the histogenetic differentiation begins. It would be easy to multiply such examples a thousandfold.

The consideration of the role of rejuvenation in the origin of organs leads us to the theory of POST-SELECTION (Nachauslese). The theory is by no means new, but I wish to emphasize its far reaching importance. The preceding discussion teaches us to divide the origin of a new morphological part into two stages. The first stage is the development of the rudiment (*anlage*) by multiplication of the cells. The second stage is characterized by the gradual differentiation of the cells, by which they become capable of their ultimate functions. Especially in embryos is the difference in time very marked between the formation and the differentiation of the "*Anlage.*" Now it is evident that the undifferentiated "*Anlage*" is not useful, but becomes useful later. The formation and conservation of the "*Anlage,*" therefore, are due to selection, working, not directly upon the "*Anlage,*" but indirectly through preservation of the fully developed organ. The conception advanced is very simple and appears to me a necessary consequence of our knowledge. For the conception itself there has been hitherto to no definite term, I propose, therefore, to call it "*Post-selection*" (in German, "*Nachauslese*). To avoid possible misunderstanding, I give another example of post-selection. A parasitic wasp lays its egg in a certain caterpillar; the mother wasp gains no advantage, natural selection does not touch her, but only her progeny, the wasp larva. Nevertheless, the survival of the fittest rules.

In conclusion, I should like to direct the reader's attention to a problem which, so far as I am acquainted with the literature of biology, has been left almost unconsidered. This present translation enables me to insert a qualification of the preceding sentence, which ought to have been inserted in the original article, namely, that the problem has been the subject of important discussions by Hyatt, Cope and a very few others among paleontologists. I am glad to be able to refer to the article by Professor Hyatt, (see Jan. Naturalist) and presents the paleontological theory of the loss of ancestral characteristics. The problem above referred to is the *problem of lost characteristics,* which seems to me one of the fundamental problems of the doctrine of evolution, because we cannot un-

derstand the development of the higher organisms until this problem is solved. Everybody is writing about the origin of new organs, and we take lively pleasure in discussions about acquired characteristics. But if we consider the circumstances closely, we recognize that the loss of ancestral characteristics almost equals in importance the acquisition of new characteristics for the formation of new species. We assume that man had fishlike ancestors, and we strengthen ourselves in this belief by the comparison so often made between the human embryo, on the one side, and the adult fish on the other. But if the comparison be impartial we are forced to admit that nearly everything which is most characteristic of the fish is conspicuously lacking in the human embryo. Taking the embryo at the stage when the gill clefts have their maximum development, we find the following relations: the body is not straight but coiled up, and this coiling up is indispensable, in order to bring about the proper distribution of the human nerves, blood vessels, and so forth; the gill clefts are closed; gills are wanting; the digestive canal has no glands; the epidermis has no scales; the chorda dorsalis does not form a large axis of the body, but is a minute string of cells. In short, the *Biogenetisches Grundgesetz* (Recapitulation theory or von Baer's law, according to Adam Sedgwick) is scarcely half true. I have previously defended this conclusion at a meeting of the American Society of Morphologists, in December, 1893, Subsequently, but independently, Adam Sedgwick has reached a similar conclusion, see his paper "*On the Law of Development, etc.*" (Quart. Jour. Micros. Sci., XXXVI, 35). Were it not, as above implied, that the departures from the fish type are in great excess, there would be no embryo at all, and consequently no man, for the adult form is a consequence of the embryonic. The embryo is the mechanical cause of the adult body. How has the disappearance of the ancestral fish characteristics been effected? The question remains unanswered. It will, perhaps, be replied "through disuse" or "through panmixia." But "disuse" is merely a name, not an explanation of the phenomena. Panmixia is an hypothesis erected on nothing. In fact, this hypothesis as-

sumes that the majority of variations fall below the value maintained by natural selection, and consequently that when the influence of natural selection is eliminated (as in disuse), the mere variation will bring the traits concerned to disappearance. It marks Weismann's style of thought to find that he has entirely omitted to determine whether his assumption was correct, and nevertheless in his book, "The Germ Plasm," presents panmixia as an established law. As a matter of fact, the statistics of variations which we already have, show that his assumption is erroneous, and that it is equally probable that mere variation will magnify a characteristic as it is that it will diminish it.

Let us return to the embryo. The following hypothesis may be advanced:

The loss of ancestral characteristics in the embryo is due to post-selection, the cells being kept in a rejuvenated stage, in order that they may afterwards accomplish new differentiations.

This conclusion follows directly from the preceding considerations, and, therefore, needs no further defense.

IV. Concluding Remarks.

The views presented in the preceeding sections are intimately connected one with another and collectively determine our conception of the process of heredity. The conception concerns only the process and not the essential character or cause of heredity. According to my view, heredity exists in all cells, but its display is inhibited by organization of the living substance, and can be complete only in embryonic cells; embryonic cells arise under very various conditions. That which is novel in this theory is the significance attributed to embryonic cells. Embryonic cells I prefer to designate as rejuvenated cells.

The theory above presented is an unavoidable consequence of the facts known, and stands in absolute contradiction with Weismann's theory of the germ plasm.

I have read with the greatest conscientiousness every article hitherto published by Weismann, which deals with his theories of heredity. My final impression from this study is that

the "*Theory of Germ Plasm*" corresponds to the personal inclinations of its author and is in no sense a logical deduction won by the collation of facts. The assumption of a difference between germ plasm and histogenic plasm explains nothing. Even according to Weismann's own exposition it explains nothing, for the supposed phenomena which the assumption is said to explain, according to Weismann, do not exist. According to him, the circumstances are the following: The phenomena due to the germ plasm do not occur in somatic cells, therefore they have a different plasm, namely, histogenic; further, these phenomena do occur in somatic cells, therefore, they have germ plasm. Attention must be directed also, and explicitly, to the fact that Weismann offers no *observations* to support his fundamental assumption. His theory is mystical to an extreme degree. In Weismann's book, "*The Germ plasm*," one finds one hypothesis after another in order to support his tottering first hypothesis—germ plasm and histogenic plasm are special and separate substances. I demand of Weismann that he lay aside *all his hypotheses*, and present to us solely the *facts*, which support his theory of germ plasm. Then he will learn, as other investigators have already learned, that his hypothesis has been built up without sufficient foundation.

Let an investigator enquire for a possibility of testing the existence of the "Ids," "Biophors," "Determinants," etc., asserted by Weismann, and he will discover that the whole fabric is woven by speculative imagination. Confirmation of his ideas has, strictly speaking, not been attempted by Weismann. Indeed, confirmation is altogether impossible, for his conceptions are far beyond the limits of present human means of investigation.

It is time to finally discard a theory which leads astray and which, although it arose without scientific justification, is again and again pushed to the front by its promulgator. It is a scientific duty to take an unhesitating stand against Weismann's theory, for only so can it become known that those who have specially occupied themselves with the problem of heredity reject Weismann's theory of germ plasm unconditionally.

APPENDIX.

The Theory of Panplasm.

It appears desirable that the modern theory of heredity should be designated by a brief and appropriate name, and accordingly I propose the term "*Panplasm*," and that the theory be called "The Theory of Panplasm." By panplasm will be understood the physical basis of hereditary transmission, which is supposed to be distributed through all cells, and which accounts for the phenomena of sexual reproduction, regeneration and asexual reproduction. Panplasm is not a collection of gemmules or biophors. The term "panplasm" was first used by me at a meeting of the Society of Arts, in Boston, November 14, 1895.

On another occasion I hope to discuss the theories of pangenesis and panplasm in their historical aspects.

THE FORMULATION OF THE NATURAL SCIENCES.[1]

By E. D. Cope.

Formulation is the method of presentation of the forms of our thoughts. Our observations of the facts of material nature are embodied in such classifications as we think best express their relations, and by means of these classifications expressed in language, we convey to others our conclusions in the premises. As the vehicle of presentation, formulation is one of the aspects of language, which as the medium of communication between men, enables them to accumulate knowledge. It is highly important then that the system of formulation should be uniform, so as to convey definite meaning and preserve the truth. The vast number of facts to be marshaled in orderly array, which constitute the natural sciences, require a

[1] Presidential address delivered before the American Society of Naturalists in Philadelphia, Dec. 26th, 1895.

correspondingly complex and exact formulation. The advent of the doctrine of evolution into the organic sciences involves the necessity of making such readjustments of our method of formulation as may be called for. It is with reference to this condition and the present action of naturalists regarding it, that I address you to-day. The subject may be considered under the three heads of Taxonomy, Phylogeny, and Nomenclature.

I. Taxonomy.

Taxonomy or classification is an orderly record of the structural characters of organic beings. The order observed is an order of values of these characters. Thus we have what we call specific or species value, generic value, family value, and so on. These values are not imaginary or artificial, as some would have us believe, but they are found in nature. Their recognition by the naturalist is a matter of experience, and the expression of them is a question of tact. Their recognition rests on a knowledge of morphology, or the knowledge of true identities and differences of the parts of which organic beings are composed. The formulation of these values in classification foreshadows the evolutionary explanation of their origin, and is always the first step necessary to the discovery of a phylogeny.

Taxonomy, then, is, and always has been, an arranging of organic beings in the order of their evolution. This accounts for the independence of the values of taxonomic characters, of any other test. Thus, no character can be alleged to be of high value because it has a physiological value, or because it has no physiological value. A physiological character may or may not have a taxonomic value. The practiced taxonomist finds a different test of values, which is this. He first endeavors to discover the series of organic forms which he studies. He learns the difference between its beginning and its ending. His natural divisions are the steps or stages which separate the one extremity from the other. The series may be greater or they may be lesser, i. e., more or less comprehensive, and it is to the series of different grades that we give the different names of the genus, family, order, etc.

We know that the characters of specific value in given cases are usually more numerous than those of higher groups. We know that they are matters of proportions, dimensions, textures, patterns, colors, etc., which are many. The characters of the higher groups, on the contrary, are what we call structural, i. e., the presence, absence, separation or fusion of elemental parts, as estimated by a common morphologic standard; and it is the business of the morphologist to determine each case on this basis. In these characters lies the key to the larger evolution, that of the higher aggregations of living things. On the contrary, the study of the origin of species characters gives us the evolution of species within the genus, but of nothing more, except by inference.

Classification, then, is a record of characters, arranged according to their values. There still lingers, in some quarters, a different opinion. This holds that there is such a thing as a "natural system," as contrasted with "an anatomical system." Examination shows that the supporters of this view suppose that there is some bond of affinity between certain living beings which is not expressed in anatomical characters. A general resemblance apparent to the eye is valued by them more highly than a structural character. If this "general appearance" is analyzed, however, it is found to be simply an aggregate of characters usually of the species type, which by no means precludes the presence of anatomical differences. And these anatomical differences may indicate little relationship, in spite of the general resemblance of the species concerned, or they may have only the smallest value attached to such characters, i. e., the generic. It is with regard to the generic characters that the chief difference of practice exists. But it is clear that the record of this grade of characters cannot be modified by questions of specific characters. The two questions are distinct. Both represent nature, and must be formulated. In fact, I have long since pointed out that the same species, so far as species characters go, may have different generic characters in different regions. Also that allied species of different genera may have more specific characters in common than remote species of the same genus.

The anticipation naturally intrudes itself that the characters which distinguish the steps in a single evolutionary or genealogical line must disappear with discovery, and new ones appear, and that they must be all variable at certain geological periods, and hence must become valueless as taxonomic criteria. And it is therefore concluded that our systematic edifice must lose precision and becomes a shadow rather than a reality. I think that as a matter of fact this will not be the result, and for the following reasons. In the first place, when, say all the generic forms of a genealogical line, shall have been discovered, we will find that each one of them will differ from its neighbor in one character only. This naturally follows from the fact that two characters rarely, if ever, appear and disappear contemporaneously. Hence, generic characters will not be drawn up so as to include several points. For a while, there will be found to be combinations of two or three characters which will serve as definitions, but discovery will relegate them to a genus each. Each of these characters will be found to have what I have called the "expression point," or the moment of completeness, before which it cannot be said to exist. In illustration I cite the case of the eruption of a tooth. Before it passes the line of the alveolus it is not in use; it is not in place as an adult organism. When it passes that line it has become mature, has reached its expression point, comes into functional use, and may be counted as a character. Such will be found to be the case with all separate parts; there always will be a time when they are not completed, and then there will be a time when they are. These lines, then, will always remain as our boundaries, as they are now, for all natural divisions from the generic upwards. This condition cannot exist in characters of proportionate dimensions, which which will necessarily exhibit complete transitions in evolution. Hence, proportions alone can only be used ultimately as specific characters.

Some systematists desire to regard phyletic series as the only natural divisions. This may be the ultimate outcome of paleontologic discovery, but at present such a practice seems to me to be premature. In the first place, as all natural divisions

rest on characters, we must continue to depend on their indications, no matter whether the result gives us phyletic series or not. In the next place, we must remember that we have in every country interruptions in the sequence of the geological formations, which will give us structural breaks until they are filled. There are also periods when organic remains were not preserved; these also will give us interruptions in our series. So we shall have to adhere to our customary method without regard to theory, and if the phyletic idea is correct, as I believe it to be, it will appear in the final result, and at some future time.

Authors are frequently careless in their definitions. Very often they include, in the definition of the order, characters which belong in that of the family, and in that of the family those that belong in the genus. Characters of different values are thus mixed. The tendency, especially with naturalists who have only studied limited groups, is to overestimate the importance of characters. Thus the tendency is to propose too many genera and other divisions of the higher grades. In some groups structure has been lost sight of altogether, and color patterns, dimensions, and even geographical range, treated as characters of genera. As the mass of knowledge increases, however, the necessity for precision will become so pressing that this kind of formulation will be discarded, and definitions which mean something will be employed. Search will be made especially for that one character which the nature of the series renders it probable will survive, as discoveries of intermediate forms are successively made, and here the tact and precision of the taxonomist has the opportunity for exercise. In the selection of these characters, one problem will occasionally present itself. The sexes of the same species sometimes display great disparity of developmental status, sometimes the male, but more frequently the female, remaining in a relatively immature stage, or in others presenting an extraordinary degeneracy. In these cases the sex that displays what one might call the genius, or in other words, the tendency, of the entire group, will furnish the definitions. This will generally be that one which displays the most numerous char-

acters. In both the cases mentioned the male will furnish these rather than the female; but in a few cases the female furnishes them. The fact that both sexes do not present them does not invalidate them, any more than the possession of distinct reproductive systems would refer the sexes to different natural divisions.

I have seen characters objected to as of little value because they were absent or inconstant in the young. I only mention the objection to show how superficially the subject of taxonomy may be treated. So that a character is constant in the adult, the time of its appearance in development is immaterial in a taxonomic sense, though it may have important phylogenetic significance.

II. Phylogeny.

The formulation of a phylogeny or genealogy involves, as a preliminary, a clear taxonomy. I refer to hypothetical phylogenies, such as those which we can at present construct are in large part. A perfect phylogeny would be a clear taxonomy in itself, so far as it should go, did we possess one; and such we may hope to have ere long, as a result of paleontological research. But so long as we can only supply parts of our phyletic trees from actual knowledge, we must depend on a clear analysis of structure as set forth in a satisfactory taxonomy, such as I have defined above.

Confusion in taxonomy necessarily introduces confusion into phylogeny. Confusion of ideas is even more apparent in the work of phylogenists than in that of the taxonomists, because a new but allied element enters into the formulation. It is in the highest degree important for the phylogenist, whether he be constructing a genealogic tree himself or endeavoring to read that constructed by some one else, to be clear as to just what it is of which he is tracing the descent. Is he tracing the descent of species from each other, or of genera from each other, or of orders from each other, or what? When I trace the phylogeny of the horse, unless I specify, it cannot be known whether I am tracing that of the species *Equus caballus*, or that of the genus Equus, or that of

the family Equidæ. When one is tracing the phylogeny of species, he is tracing the descent of the numerous characters which define a species. This is a complex problem, and but little progress has been made in it from the paleontologic point of view. Something has been done with regard to the descent of some living species from each other. But when we are considering the descent of a genus, we restrict ourselves to a much more simple problem, i. e., the descent of the few simple characters that distinguish the genus from other genera. Hence, we have made much more progress in this kind of phylogeny than with that of species, especially from the paleontologic point of view. The problem is simplified as we rise to still higher divisions, i. e., to the investigation of the origin of the characters which define them. We can positively affirm many things now as to the origin of particular families and orders, especially among the Mammalia, where the field has been better explored than elsewhere.

It is in this field that the unaccustomed hand is often seen. Supposing some phyletic tree alleges that such and such has been the line of descent of such and such orders or families, as the case may be; soon a critic appears who says that this or that point is clearly incorrect, and gives his reasons. These reasons are that there is some want of correspondence of generic characters between the genera of the say two families alleged to be phyletically related. And this want of correspondence is supposed to invalidate the allegation of phyletic relation between the families. But here is a case of irrelevancy; a generic character cannot be introduced in a comparison of family characters. In the case selected, the condition is to be explained by the fact that although the families are phyletically related, one or both of the two juxtaposed genera through which the transition was accomplished has or have not been discovered. The same objection may be made against an allegation of descent of some genus from another, because the phyletic relation between the known species of the two genera cannot be demonstrated. I cite as an example the two genera, Hippotherium and Equus, of which the latter has

been asserted with good reason to have descended from the former. It has been shown, however, that the *Equus caballus* could not have descended from the European *Hippotherium mediterraneum*, and hence some writers have jumped to the conclusion that the alleged phyletic relation of the two genera does not exist. The reasons for denying this descent are, however, presented by specific characters only, and the generic characters are in no way affected. Further, we know several species of Hippotherium which could have given origin to the *Equus caballus* probably through intermediate species of Equus.

Some naturalists are very uncritical in criticising phylogenies in the manner I have just described. They often neglect to ascertain the definitions given by an author to a group alleged by him to be ancestral; but fitting to it some definition of their own, proceed to state that the ancestral position assigned to it cannot be correct, and to propose some new division to take its place. It is necessary to examine, in such cases, whether the new group so proposed is not really included in the definition of the old one which is discarded.

The fact that existing genera, families, etc., are contemporary need not invalidate their phyletic relation. Group No. 1 must have been contemporary with group No. 2, at the time that it gave origin to the latter, and frequently, though always, a certain number of representatives of group No. 1 have not changed, but have persisted to later periods. Some genera, as, e. g., Crocodilus, have given origin to other genera (i. e., Diplocynodon) and have outlasted it, for the latter genus is now extinct. The lung fishes, Ceratodus, are probably ancestral to the Lepidosirens, but both exist to-day. Series of genera, clearly phyletic, of Batrachia Salientia, are contemporaries. Of course we expect that the paleontologic record will show that their appearance in time has been successive. But many ancestors are living at the same modern period as their descendents, though not always in the same geographic region.

III. Nomenclature.

Nomenclature is like pens, ink and paper; it is not science, but it is essential to the pursuit of science. It is, of course, for convenience that we use it but it does not follow from that that every kind of use of it is convenient. It is a rather common form of apology for misuse of it to state that as it is a matter of convenience, it makes no difference how many or how few names we recognize or use. An illustration of this bad method is the practice of subdividing a genus of many species into many genera, simply because it has many species. The author who does this ignores the fact that a genus has a definite value, no matter whether it has one or five hundred species. I do not mean to maintain that the genus or any other value has an absolute fixity in all cases. They undoubtedly grade into each other at particular places in the system, but these cases must be judged on their own merits. In general there is no such gradation.

Nomenclature is then orderly because the things named have definite relations which it is the business of taxonomy, and nomenclature its spokesman, to state. Here we have a fixed basis of procedure. In order to reach entire fixity, a rule which decides between rival names for the same thing is in force. This is the natural and rational law of priority. With the exception of some conservative botanists, all naturalists are, so for as I am aware, in the habit of observing this rule. The result of a failure to do so is self evident. There is, however, some difference of opinion as to what constitutes priority. Some of the aspects of the problem are simple, others more difficult. Thus there is little or no difference of opinion as to the rule that the name of a species is the first binomial which it received. This is not a single date for all species, since some early authors who used trinomials and polynomials occasionally used binomials. A second rule which is found in all the codes, is that a name in order to be a candidate for adoption, must be accompanied by a descriptive diagnosis or a plate. As divisions above species cannot be defined by a plate, a description is essential in every such case.

It is on the question of description that a certain amount of difference of opinion exists. From the codes of the Associations for the Advancement of Science, and of the Zoological Congresses, no difference of opinion can be inferred, but the practice of a number of naturalists both zoologists and paleontologists in America, and paleontologists in Europe, is not in accord with the rule requiring definition of all groups above species. It has always appeared to me remarkable that a rule of such self evident necessity should not meet with universal adoption. However, the objections to it, such as they are, I will briefly consider. It is alleged that the definitions when first given are more or less imperfect, and have to be subsequently amended, hence it is argued they have no authority. However, the first definitions, if drawn up with reference to the principles enumerated in the first part of this address, need not be imperfect. Also an old time diagnosis of a division which we have subsequently found it necessary to divide, is not imperfect on that account alone, but it may be and often is, the definition of a higher group. But you are familiar with all this class of objections, and the answers to them, so I will refer only to the positive reasons which have induced the majority of naturalists to adhere to the rule.

It is self evident that so soon as we abandon definitions for words, we have left science and have gone into a kind of literature. In pursuing such a course we load ourselves with rubbish, and place ourselves in a position to have more of it placed upon us. The load of necessary names is quite sufficient, and we must have a reason for every one of them, in order to feel that it is necessary to carry it. Next, it is essential that every line of scientific writing should be intelligible. A man should be required to give a sufficient reason for everything that he does in science. Thus much on behalf of clearness and precision. There is another aspect of the case which is ethical. I am aware that some students do not think that ethical considerations should enter into scientific work. To this I answer that I do not know of any field of human labor into which ethical considerations do not necessarily enter. The reasons for sustaining the law of priority are partly

ethical, for we instinctively wish to see every man credited with his own work, and not some other man. The law of priority in nomenclature goes no further in this direction than the nature of each case requires. Nomenclature may be an index of much meritorious work, or it may represent comparatively little work; but it is to the interest of all of us that it be not used to sustain a false pretence of work that has not been done at all. By insisting on this essential test of honest intentions we retain the taxonomic and phylogenetic work within the circle of a class of men who are competent to it, and cease to hold out rewards to picture makers and cataloguers.

Another contention of some of the nomenclators who use systematic names proposed without description, is, that the spelling in which they were first printed must not be corrected if they contain orthographical and typographical errors. That this view should be sustained by men who have not had the advantage of a classical education, might not be surprising, although one would think they would prefer to avoid publicly displaying the fact, and would be willing to travel some distance in order to find some person who could help them in the matter of spelling. But when well educated men support such a doctrine, one feels that they have created out of the law of priority a fetish which they worship with a devotion quite too narrow. The form of our nomenclature being Latin, the rules of Latin orthography and grammar are as incumbent on us to observe, as are the corresponding rules of English grammar in our ordinary speech. This cult so far as I know, exists only in the United States and among certain members of the American Ornithologists Union. The preservation of names which their authors never defined; of names which their proposers misspelled; of names from the Greek in Greek instead of Latin form; of English hyphens in Latin composition; and of hybrid combinations of Greek and Latin, are objects hardly worth contending for. Some few authors are quite independent of rules in the use of gender terminations, but I notice the A. O. U. requires these to be printed correctly. Apart from this I notice in the second edition of their check list of North American Birds just issued, only

eighteen misspellings out of a total number of 768 specific and subspecific names, and the generic and other names accompanying. These are of course not due to ignorance on the part of the members of this body, some of whom are distinguished for scholarship, but because of an extreme view of the law of priority.

In closing I wish to utter a plea for euphony and brevity in the construction of names. In some quarters the making of such names is an unknown art. The simple and appropriate names of Linneus and Cuvier can be still duplicated if students would look into the matter. A great number of such names can be devised by the use of significant Greek prefixes attached to substantiatives which may or may not have been often used. Personal names in Greek have much significance, and they are generally short and euphonious. The unappropriated wealth in this direction is so great that there is really no necessity for poverty in this direction. It should be rarely necessary, for instance, to construct generic names by adding prefixes and suffixes of no meaning to a standard generic name already in use.

SOME LOCALITIES FOR LARAMIE MAMMALS AND HORNED DINOSAURS.

By J. B. Hatcher.

It is the purpose of this paper to give brief but accurate descriptions of the localities for the most important and best preserved specimens of Laramie mammals and horned and other dinosaurs collected by the writer for the U. S. Geological Survey, and now carefully stored in the Yale Museum at New Haven; with a map of the most important locality at present known and suggestions to collectors visiting this, or other localities as to the most promising places and best methods to be employed in order to attain the greatest degree of success.

History of the Discovery of Laramie Horned Dinosaurs.

As early as 1872, Professor Cope[1] described under the name of *Agathaumas sylvestre* a portion of the skeleton of a horned dinosaur from Laramie beds near Black Butte in southwestern Wyoming. In various publications from 1874–1877 which appeared in THE AMERICAN NATURALIST, Proceedings Philadelphia Academy Sciences and Bulletins of the U. S. Geological Survey, Cope has added much to our knowledge of these strange forms, chiefly from material collected by himself and Mr. Charles H. Sternberg from the vicinity of Cow Island on the upper Missouri River in Montana.

In 1887 a new locality for horned dinosaurs was found near Denver, Colorado, by Mr. George L. Cannon. The most important specimen, consisting of a pair of horn cores, was sent to Professor Marsh for identification and description. They were not characteristic, and owing to their striking resemblance to the horns of certain fossil Bisons, they were referred by Marsh to that genus and described under the name of *Bison alticornis;* the beds in which they were found being referred to late Pliocene and denominated the *Bison beds.*[2]

In 1888 the writer secured in the same locality in which Cope had operated in 1875 and 1876 on the upper Missouri, parts of several skulls of a horned dinosaur, some of which Marsh has described, creating for them a new genus *Ceratops*, and several new species. A comparison of the types of Cope's *Monoclonius recurvicornis* and Marsh's *Ceratops montanus*, both from the same locality in Montana, would doubtless establish the generic identity of the two.

Not until 1889 was a locality found where remains of these animals were sufficiently abundant and well preserved to afford material which would give us an adequate idea of their structure and habits. In the fall of 1888 the writer's attention was called to a pair of horncores belonging to Mr. C. A. Guernsey, of Douglas, Wyoming. Upon inquiry it was learned that they had been taken from a huge skull found by Mr. E. B. Wilson

[1] Proc. Am. Phil. Soc., 1872, p. 482.

[2] See notice of new Fossil Mammals, Am. Jour. Sci., Oct., 1887.

on Buck Creek, some of 35 miles north of Lusk, Wyoming. Early in the spring of 1889 the writer proceeded to Lusk, near which place Mr. Wilson still lived, and easily succeeded in getting that most accomodating gentleman to show him the skull from which he had taken the horns. This has proved a most important locality, and material obtained from it has increased many fold our knowledge of the Laramie reptilian and mammalian faunas. In the nearly four years spent by the writer in working these beds, 31 skulls and several fairly complete skeletons of horned dinosaurs were secured, besides two quite complete skeletons of *Diclonius* (*Claosaurus*), about 5000 isolated jaws and teeth of Laramie mammals and numerous remains of other dinosaurs, turtles, lizards, birds and fishes, as well as extensive collections of freshwater invertebrates from the same beds. In all over 300 large boxes of fossils were collected for the U. S. Geological Survey, and are now carefully stored in the Yale Museum, many of them as yet unopened.

At present remains of horned dinosaurs are known from only four widely separated localities; one of these, that of Black Butte, Wyoming, is west of the main range of the Rocky Mountains, and the other three including the Denver locality in Colorado; the Converse Co. locality in the extreme eastern portion of central Wyoming, and the Judith River or Cow Island locality in northern Montana, lie east of the main range. There are other localities known to the writer, but they are as yet of minor importance, since little collecting has been done in them and no material has been described from them. They will be referred to later.

The Ceratops Beds.

In the American Journal of Science for December, 1889, Professor Marsh applied the name *Ceratops beds* to certain strata in the west from which horned dinosaurs had been secured. He did not then, nor has he at any time since, designated just what he considered the geographical distribution of these beds nor their upper and lower delimitations in the geological scale. In order that the reader may not be misled in regard to Professor Marsh's position on this question I will quote him some-

what fully. In speaking of the horned dinosaurs in the publication just cited he says: "The geological deposits, also, in which their remains are found have been carefully explored during the past season, and the known localities of importance examined by the writer, to ascertain what other fossils occur in them, and what were the special conditions which preserved so many relics of this unique fauna.

"The geological horizon of these strange reptiles is a distinct one in the upper Cretaceous, *and has now been traced nearly eight hundred miles along the eastern flank of the Rocky Mountains.* It is marked almost everywhere by remains of these reptiles, and hence the strata containing them may be called the Ceratops beds. They are freshwater or brackish deposits, which form a part of the so-called Laramie, *but are below the uppermost beds referred to that group.* In some places, at least, they rest upon marine beds which contain invertebrate fossils characteristic of the Fox Hills deposits." Italics mine.

If we accept literally Marsh's statement that the Ceratops beds have been traced for eight hundred miles along the eastern flank of the Rocky Mountains, it will be necessary to suppose that he includes in the Ceratops beds not only the beds in Converse Co., Wyoming, but also the *Bison beds* (*Denver beds of Cross*) at Denver, and the *Judith River beds* on the upper Missouri. These are very widely separated localities, and no attempt has ever been made to trace the continuity of the strata from the one to the other, nor is it at all probable that such an attempt would meet with success. Professor Marsh did in the autumn of 1889 spend nearly two days in the Converse Co. locality, and again in 1891 he spent one full day in the same locality; but his time was occupied in visiting a few of the localities in which dinosaur skulls and skeletons and Laramie mammals had been found. No time was taken to determine the upper and lower limits of the beds or to trace the outcrops of the strata. After his visit in 1889 when he spent nearly two days with our party in the Converse Co. locality, he took the train for Denver, and in the company of Mr. George L. Cannon of that city, he spent one-half day examining the *Bison beds* (Denver beds). This constitutes Professor

Marsh's *field work* in the *Ceratops beds.* In a total of three and one-half days field work he seems to have found sufficient time to "carefully explore" the geological deposits of the *Ceratops beds* and to trace them for "eight hundred miles along the eastern flank of the Rocky Mountains," besides making numerous other observations of scientific interest.

Of the many interesting vertebrate fossils described by Professor Marsh from the *Ceratops beds,* those from the Denver locality were secured by Messrs. Cross, Eldridge and Cannon, and those from Wyoming and Montana by the writer or men in his party, with one exception only, namely, the type of *Hadrosaurus breviceps,* which was received at New Haven many years ago, the locality on the label accompanying it being given as Bear Paw Mountains, Montana, which is of course incorrect, it doubtless is from the vicinity of Cow Island. With this one single exception I can confidently state that all the material described by Professor Marsh as from the Laramie or Ceratops beds of Wyoming is, without exception, from Converse Co., and was found within an area not exceeding fifteen miles in width from east to west by thirty miles in length from north to south; and all the material described by him as from Montana, with the one exception mentioned, was found on the Missouri River between the mouth of Arrow Creek, just above Judith River, and the mouth of Cow Creek, some forty-five miles below, and never back farther than ten miles from the Missouri. It will thus be seen that the actually known area of the *Ceratops beds* is indeed very limited, and from these areas we should exclude certainly, the Judith River or upper Missouri and very likely the Black Butte locality in southwestern Wyoming. The beds of the former certainly and those of the latter almost certainly, belong to an older horizon than those of the Denver or Converse Co. localities; the latter may be considered as the typical locality for the Ceratops beds. All of the dinosaurs from the Judith River country are smaller, less specialized forms than those from the Converse Co. and Denver localities, as has already been observed by Marsh.

Marsh's statements that the Ceratops beds are below the uppermost beds referred to the Laramie and that they rest upon

marine beds which contain invertebrate fossils characteristic of the Fox Hills deposits, may well be questioned, especially if we exclude from the *Ceratops beds* the *Judith River beds* and refer them to a lower horizon, retaining for them the name *Judith River beds.* At no place in the Converse Co. region do the *true Ceratops beds,* with remains of horned dinosaurs, rest upon true marine Fox Hills sediments; nor are the Ceratops beds in this region overlaid by strata which could be referred without doubt to the Laramie. The writer has, in a paper published in the American Journal of Science of February, 1893, stated that the *Ceratops beds* rest directly upon the *Fox Hills* series, and has provisionally referred the very similar series of sandstones and shales conformably overlying the *Ceratops beds* to the upper Laramie; but it would doubtless be better to restrict the limits of the Ceratops beds to those strata in which horned dinosaurs occur, and to consider the underlying 400 feet of barren sandstones as the equivalent of the *Judith River beds.* Future investigations will doubtless show that the sandstones, shales and lignites overlying the typical Ceratops beds in Converse Co. should be referred to the *Fort Union beds* and not to the Laramie, as, according to Knowlton, the limited flora sent him now indicates.

The terms Fox Hills and Laramie as now used cannot be taken to represent distinct and different periods of time. for as has been shown by G. M. Dawson, Selwyn and McConnell in the Belly River region in Canada, and frequently observed by the writer on the upper Missouri in Montana, marine beds with typical Fox Hills fossils have been found interstratified with fresh and brackish water beds containing characteristic Laramie fossils, showing conclusively that the two periods were in part at least contemporaneous; the one representing the marine and the other fresh or brackish water forms existing at the same time and in not widely separated regions, these alternations in the nature of the fauna in the same locality having been brought about by successive encroachments and recessions of the sea. It is not at all impossible that in the region of Converse Co., Wyoming, marine conditions prevailed continuously until late in Laramie times, and that during the

time of the deposition of the Laramie beds of the Judith River and Black Butte localities, marine beds with Fox Hills types of fossils were being deposited over the region of what is now eastern and central Wyoming. This would account for the absence in this region, of the lower Laramie, with the smaller and less specialized horned, and other dinosaurs, characteristic of it.

Other Localities for Horned Dinosaurs.

In addition to the localities already mentioned, the writer has seen remains of horned dinosaurs and Hadrosauridæ on the North Platte River opposite the mouth of the Medicine Bow, about 35 miles below Fort Steele, Wyoming; along the eastern flank of the Big Horn Mountains about 40 miles south of Buffalo, Wyoming; on the west side of the Big Horn River between Fort Custer and Custer Station, Montana; and on Willow Creek 13 miles north of Musselshell postoffice in Montana. Another region not examined, but which looked very promising, is near the town of Havre on the Great Northern Railroad just north of the Bear Paw Mountains in northern Montana.

Suggestions to Collectors.

Of all the localities for Laramie dinosaurs and mammals known to the writer, that of Converse Co., Wyoming, is by far the most promising, and the earnest and intelligent collector will there meet with a fair amount of success for many years to come. As will be seen, by reference to the map accompanying this paper, this region is easy of access from the town of Lusk on the F. E. and M. V. R. R. At this station all necessary camp supplies can be obtained. The fossil beds are easily worked, the country being quite open and its surface not disfigured by deep and impassable cañons. There is abundant grass for horses, wood for fuel, and frequent small springs of fairly good water.

Vertebrate fossils are never abundant in the Laramie, but every exposure in this region should be carefully searched and especially the large sandstone concretions which contain many

of the best skulls and skeletons found in these beds. Fossils are found in both the shales and sandstones, but are best preserved in the latter. The small mammals are pretty generally distributed but are never abundant, and on account of their small size are seen with difficulty. They will be most frequently found in what are locally known as "blow outs" and are almost always associated with garpike scales and teeth, and teeth and bones of other fish, crocodiles, lizards and small dinosaurs. These remains are frequently so abundant in "blow outs" as to easily attract attention, and when such a place is found careful search will almost always be rewarded by the discovery of a few jaws and teeth of mammals. In such places the ant hills, which in this region are quite numerous, should be carefully inspected as they will almost always yield a goodly number of mammal teeth. It is well to be provided with a small flour sifter with which to sift the sand contained in these ant hills, thus freeing it of the finer materials and subjecting the coarser material remaining in the sieve to a thorough inspection for mammals. By this method the writer has frequently secured from 200 to 300 teeth and jaws from one ant hill. In localities where these ants have not yet established themselves, but where mammals are found to be fairly abundant it is well to bring a few shovels full of sand with ants from other ant hills which are sure to be found in the vicinity, and plant them on the mammal locality. They will at once establish new colonies and, if visited in succeeding years, will be found to have done efficient service in collecting mammal teeth and other small fossils, together with small gravels, all used in the construction of their future homes. As an instance of this, I will mention that when spending two days in this region in 1893, I introduced a colony of ants in a mammal locality, and on revisiting the same place last season I secured in a short time from the exterior of this one hill 33 mammal teeth.

Another way to secure these small teeth is to transport the material to a small stream and there wash it in a large sieve in the water, the finer material being washed away, but this treatment is too harsh to give the best results, what few jaws there are always being broken to bits.

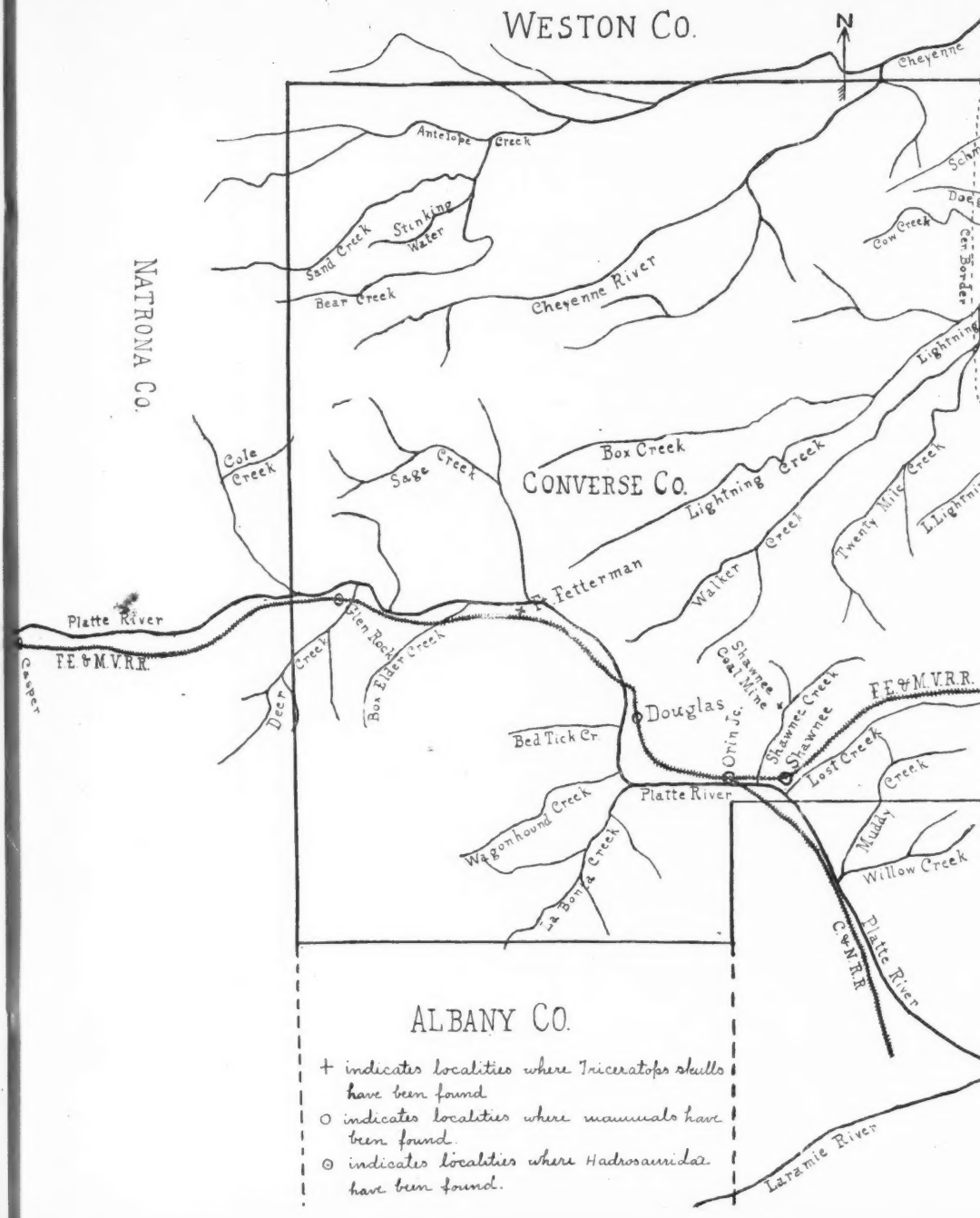
In the map accompanying this paper the dotted line marked Cer. border, indicates approximately the boundaries of the *Ceratops beds* in Converse Co. The line to the south and east indicating the outcrop of the beds, that to the west the point where they pass under the overlying beds. They extend on uninterruptedly into Weston Co., but have not been worked farther north than Schneider Creek. A working party encamped at the mouth of Schneider Creek on the Cheyenne River, would doubtless meet with much success. The map explains itself; it was drawn by the writer to accompany his paper on the Ceratops beds above referred to, but was not then published. A copy of it was given by him to Professor Marsh and a portion of it redrawn, will appear in the 16th Annual Report of the U. S. Geological Survey accompanying a memoir by Professor Marsh.

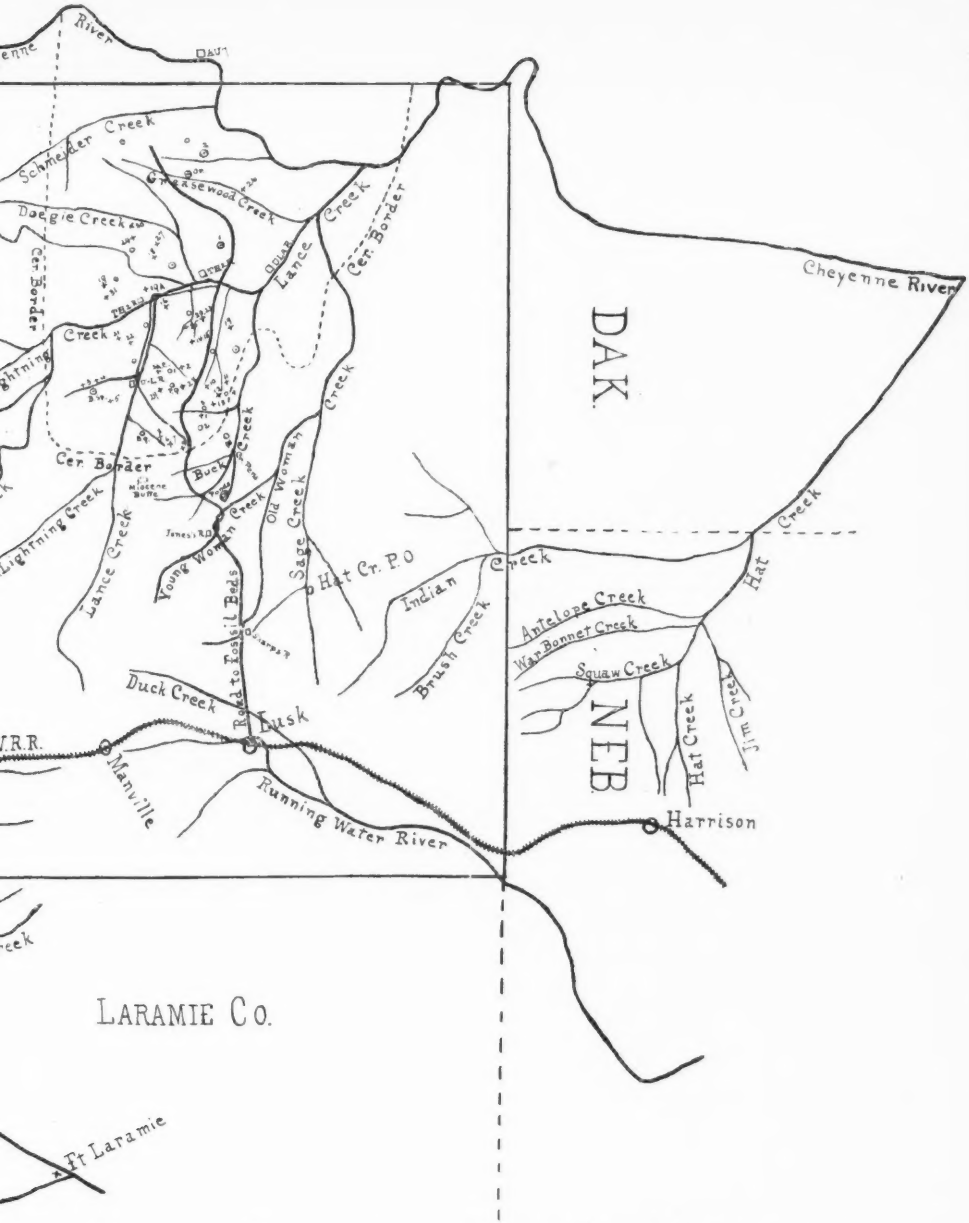
Princeton, N. J., Jan. 6, 1896.

RECENT LITERATURE.

Recent Books on Vegetable Pathology.—(1) Kirchner, Dr. Oskar: *Die Krankheiten und Beschädigungen unserer landwirtschaftlichen Kulturpflanzen.* Stuttgart, 1890, pp. VI, 637; (2) Comes, Dr. O.: *Crittogamia Agraria.* Naples, 1891, pp. 600; (3) Ward, Dr. H. Marshall: *Diseases of Plants.* London, (no date), pp. 196; (4) Ludwig, Dr. Friedrich: *Lehrbuch der niederen Kryptogamen.* Stuttgart, 1892, pp. XV, 672; (5) Tubeuf, Dr. Karl Freiherr von: *Pflanzenkrankheiten durch Kryptogame Parasiten verursacht.* Berlin, 1895, pp. XII, 599; (6) Frank, Dr. A. B.: *Die Krankheiten der Pflanzen.* Breslau, 1895–1896, Vol. 1, pp. XII, 344; Vol. II, pp. XI, 574; (7) Prillieux, Ed.: *Maladies des Plantes agricoles et des arbres fruitiers et forestiers causé par des parasites végétaux.* Paris, 1895, Vol. I, pp. XVI, 421.

It is desired only to call attention to these books at this time by means of the briefest mention. Some have been published long enough to enable one to speak freely of their merits and demerits; others are very recent additions to the literature of vegetable pathology and use has not yet demonstrated strong or weak points.


WESTON CO.
N
Cheyenne
Antelope Creek
Sand Creek
Stinking Water
Bear Creek
Cheyenne River
Cow Creek
NATRONA CO.
Cole Creek
Sage Creek
Box Creek
CONVERSE CO.
Lightning Creek
Twenty Mile Creek
Walker Creek
Fetterman
Platte River
Casper
F.E. & M.V.R.R.
Glen Rock
Box Elder Creek
Deer Creek
Douglas
Shawnee Coal Mine
Shawnee Creek
Shawnee
Orin Jc.
Bed Tick Cr
Lost Creek
Platte River
Wagonhound Creek
La Bonte Creek
Muddy Creek
Willow Creek
C. & N.R.R.
ALBANY CO.
+ indicates localities where Triceratops skulls have been found.
○ indicates localities where mammals have been found.
⊙ indicates localities where Hadrosaurida have been found.
Laramie River



MAP OF CONVERSE COUNTY, WYOMING.
SCALE ABOUT 16 MILES TO THE INCH.

Dr. Kirchner's book deals with diseases due both to animal and vegetable parasites. Its statements are reasonably accurate and it is so arranged as to greatly facilitate identification of diseases. No illustrations.

Dr. Comes' book contains quite a full account of some parasitic diseases and brief mention of many others. It was the first book of its kind to pay much attention to bacterial diseases of plants. Its statements are frequently inaccurate and the 17 plates illustrating fungi and fungous diseases are poorly executed and add nothing to the value of the book.

Prof. Ward's little book is by far the best thing in English. It discusses only a few diseases and all of these in a very elementary, popular way, but there are many interesting suggestions, and the facts which are given are usually stated accurately. There are 53 text figures and a brief index. A book of about the same size and style by the same author, on *Timber and Some of its Diseases*, (1889) makes a good companion volume.

Dr. Ludwig's book is uneven in its make up, some parts being quite free from erroneous statements and others, those dealing presumably with the subjects least familiar to the author, needing careful revision. The book certainly deserves a second edition. From the pains taken to say something about everything, it is perhaps more generally useful than any of the preceding or than the following work.

Dr. Tubeuf's book is very attractive. The type is large and clear, and the unhackneyed character of the illustrations, many of which were prepared expressly for this work, is especially commendable. The treatment of certain subjects indicates that the author depended upon imperfect reviews rather than on the original papers, e. g., Wakker's bacterial disease of hyacinths, and Mayer's mosaic disease of tobacco; but the book as a whole has not been read carefully enough to warrant any extended criticism.

Dr. Frank's book is the second revised edition of his well known handbook, *Die Krankheiten der Pflanzen*, published in 1880, and now sadly out of date. Much new matter has been added and an earnest effort made to bring the subject up to date. This has succeeded as well, perhaps, as the rapidly growing state of the science will admit. The first volume deals with non-parasitic diseases; the second with fungous parasites. Most of the figures appear to be old, and the letter press is indifferent.

Dr. Prillieux's book is attractive in appearance, but some of it is sketchy and rather unsatisfactory, and due credit is not always given.

Quite often the reader finds himself wishing the author had stated some matter exactly rather than vaguely, e. g., germination of the oospores of *Plasmopara viticola.* Prillieux is probably right in maintaining that Viala has not satisfactorily determined the aetiology of Brunnisure and the California vine disease, the microscopic appearances ascribed to a Plasmodiophora being quite as likely due to the effect of strong reagents on the protoplasm of the cell. Some of the figures in this book are excellent, others are very poor. There is no index.

It is to be hoped that Dr. Sorauer will now bring out another edition of his *Handbuch der Pflanzenkrankheiten*, or at least of the 2nd volume on parasitic plants which was issued in 1886 and needs revision badly. All of these books are useful to American students, and should certainly find place on the book shelves of every vegetable pathologist. It would seem that the time is not ripe for the appearance of standard American works on this subject. There is, however, great activity in the study of plant diseases in this country, and we may look for a crop of them within the next decade.—ERWIN F. SMITH.

The Iowa University Bahama Expedition.[1]—The history of an educational and scientific experiment is given Mr. C. C. Nutting in this octavo volume of 251 pages. It is published as Bulletins Nos. 1 and 2, Vol. III, of the laboratories of Natural History of the Iowa State University. The zoology of the region visited is treated of in a general way with a view to giving an idea of the facies of the collections from the several localities. The marine and land invertebrata are treated of quite fully, but none of the vertebrates receive much attention excepting the birds. The beauties of marine life are graphically described, and a considerable number of illustrations add to the general excellence of the get up of the book. An appendix gives a list of commissary stores actually used during the expedition.

Mr. Nutting, in summing up the results of the expedition, draws attention to the fact that this enterprise demonstrates the practicability of accomplishing such results at a cost which is merely nominal.

The Shrews of North America.[2]—The tenth number in the North American Fauna series published by the U. S. Department of Agriculture, contains three papers on the Shrews: A revision of the genera Blarina and Notiosorex by Dr. C. H. Merriam, a synopsis of the

[1] The Bahama Expedition. Bulls. Nos. 1 and 2, Vol. III, Laboratories Nat. Hist. Iowa State Univ. Iowa City 1895.

[2] North American Fauna No. 10, Washington, 1895. Comprising papers by C. Hart Merriam and G. S. Miller, Jr.

genus Sorex by the same author, and a discussion of the long-tailed Shrews of eastern United States by G. S. Miller, Jr.

In regard to the short tailed Shrews of the genus Blarina, Dr. Merriam states that up to the present time 8 valid species have been described from the United States, 2 from Mexico, 1 from Guatemala and 2 from Costa Rica. Twelve new forms are here added, 3 from the eastern United States and 9 from Mexico, making 20 members of the genus now known. The type localities are given and the geographical distribution. A complete synonomy accompanies each description.

Dr. Merriam's second paper is a synopsis of the species of Sorex, and is based on an examination of 1200 specimens. In this material were found 20 new forms which are here described. In this paper, as in the first, careful attention has been given to the synonomy.

The only genera of Soricidae included in this monograph by Dr. Merriam are Blarina Gray, Notiosorex Baird and Sorex Linn.

Mr. Miller's contribution is a study of the long tailed Shrews of the eastern United States. The author gives in detail the history of each species. The descriptions include the type locality, geographic distribution, and detailed information under the head of general remarks.

Figures of all the species described are given on 12 page plates, and they are of excellent quality. The monographs are the most important contributions to the subject that have been made, and are indispensable to the student of N. American mammalia.

Iowa Geological Survey, Vol. III.[3]—A quarto volume containing the several reports of the geological corps, with accompanying papers of the geology of special formations and areas. The work in the southwest half of the State was done under the immediate supervision of Dr. Keyes who contributes three papers on the geology of that section, and also one on the glacial scorings in Iowa. Mr. Calvin discusses the composition and origin of the Iowa Chalk. The Paleozoic strata in the northeastern part of the State, and certain Carboniferous and Devonian outliers in the eastern region are reported upon by Mr. Norton. The Cretaceous deposits of the Sioux Valley by Mr. Bain and certain buried River Channels by Mr. Gordon. The illustrations include 37 plates, a number of maps, and 34 figures in the text. We are glad to learn that the survey is in a prosperous condition, and hope that its work will be appreciated at its true worth by the State authorities.

[3] Iowa Geological Survey, Vol. III. Second Annual Report, 1895, with accompanying papers. Des Moines, 1895.

Duration of Niagara Falls and History of the Great Lakes.[4]—This work contains the researches of the author which have been published in America and Europe, on the Origin of the Great Lake Basins; Changes of Continental Altitudes; Deformation of Beaches; Glacial Dams; Births of Lakes Ontario, Erie, Huron, etc.; Changes of River Courses; and the History and Duration of Niagara Falls. It is one of the most important works on geological science that has been produced in this or any other country as an original research. It furnishes a standard of estimation of postglacial history for this continent, which must always be referred to in all questions relating to the antiquity of man, as well as those relating to the present distribution of land and water.

The text is fully illustrated with maps, section drawings, etc. One of the fine page plates which accompany the work is a reproduction from a camera obscura drawing made by Henry Ransford in 1832, the oldest accurate picture of the Falls known to the author.

The author estimates that the period which has elapsed since the falls were at Lake Ontario amounts to 32000 years.

Korean Games.[5]—In pursuance of a theory that games must be regarded as survivals from primitive conditions, under which they originated in magical rites and chiefly as a means of divination, Mr. Stewart Culin has made an extensive study of the games of Korea. He finds that there were two principal systems of divination in Eastern Asia from which games arose, in both of which the arrow or its substitute was employed as the implement of magic. Of the 97 games described in his book, 23 are directly connected with some such use of the arrow. A large number of the other games described consist of athletic sports ceremonially practiced in the sacred pavilions of Korea, and like the divatory tugofwar, still retain traces of their primeval divinatory character.

The illustrations are almost entirely by native artists, and they give the book a value altogether unique. They comprise 22 colored plates and 135 figures in the text. The subject is a very curious one, and as treated by Mr. Culin, it becomes an important guide to the history of human migrations and human thought.

[4] The Duration of Niagara Falls and the History of the Great Lakes. By J. W. Spencer. New York, Humboldt Publishing Co.

[5] Korean Games. With Notes on the Corresponding Games of China and Japan. By Stewart Culin. Philadelphia, 1895.

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General Notes.

PETROGRAPHY.[1]

Igneous Rocks of St. John, N. B.—W. N. Mathew has continued his work on the igneous rocks of St. John, N. B.,[2] contributing in a recent article an account of the effusive and dyke rocks of the region. All the rocks described are believed to be pre-Cambrian in age. They embrace quartz-porphyries, felsites, porphyries, diabases and feldspar-porphyrites among the effusive rocks, and diorite-porphyrites, diabases and augite-porphyrites among the dyke forms. In some of the quartz-porphyries perlitic cracks may still be recognized, and in the felsite porphyries some spherulites. Tuffs of all the effusives are abundant. A soda granite with augite and green hornblende and probably a little glaucophane was also met with. It is intrusive, and has a composition represented by the figures:

SiO_2	TiO_2	Al_2O_3	Fe_2O_3	FeO	MnO	CaO	MgO	Na_2O	K_2O	CO_2	Loss
64.86	.70	15.02	5.53	1.01	.18	2.61	1.42	3.92	2.37	.55	1.73

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Trans. N. Y. Acad. Science, XIV, p. 187.

The diorite-porphyrite has a groundmass of idiomorphic hornblende, lathshaped feldspars and some interstitial quartz, with phenocrysts of the same minerals, but principally of feldspar. Among the diabases is a quartzose variety.

Eruptive Rocks from Montana.—Among some specimens of eruptive rocks obtained from Gallatin, Jefferson and Madison Counties, Montana, Merrill[3] finds basalts, andesites, lamprophyres, syenites, porphyrites, wehrlites, harzburgites and websterites, some of which possess peculiar characteristics. A hornblende andesite, for instance, contains large corroded brickred pleochroic apatite crystals, whose color is due to innumerable inclusions scattered through them. The groundmass of some of the basalts has a spherulitic structure. The wehrlite is a holocrystalline aggregate of pale green diallage, reddish brown biotite, colorless olivine and a few patches of plagioclase. Its structure is cataclastic or granulitic, the larger crystals being surrounded by an aggregate of smaller ones. The websterite consists of green diallage and colorless enstatite with included foliae of mica and occasional interstitial areas of feldspar, and is thus related to gabbro. Some of the lamprophyres are composed of groups of polysomatic olivines or of olivine and augite in a scaly granular groundmass of lighter colored minerals, through which are scattered small flakes of brown biotite and tiny augite microlites. This structure is accounted for on the supposition that the granular groups of olivine and of olivine and augite belong to an older series of crystalline products than those of the groundmass.

Porphyrites and the Porphyritic Structure.—In a general account of the laccolitic mountains of Colorado, Utah and Arizona, Cross[4] gives a brief synopsis of the characteristics of the rocks that constitute their cores. These rocks comprise augite, hornblende and hornblende mica-porphyrites, diorites and quartz-porphyrites. All contain phenocrysts of plagioclase and of the iron bearing silicates, with the feldspars largely predominating. These upon separating left for consolidation into the groundmass a magma which upon crystallization yielded a granular aggregate consisting largely of quartz and orthoclase. No pressure effects were seen in any of the sections studied. All are porphyritic with a granular groundmass, which differs in the different rocks, principally in the proportion of its constituents. The porphyritic structure as defined by the author is not the result of the recur-

[3] Proc. U. S. Nat. Museum, XVII, p. 637.

[4] 14th Ann. Rep. U. S. Geol. Survey.

rence of crystallization, producing several generations of crystals, but it is a structure exhibiting contrasts in the size and form of the component crystals of a rock, resulting from the differences in conditions under which the different minerals crystallized.

Granophyre of Carrock Fell, England.—In the Carrock Fell district is a red granophyre closely associated with the gabbros. This rock has recently been studied by Harker,[5] who had previously investigated the gabbros. The normal type of the granophyre is an augitic variety in which the augite occurs as a deep green species which is idiomorphic toward the feldspars. Oligoclase is also present as idiomorphic crystals in a reddish quartz-feldspar groundmass with the typical granophyric structure. The composition of the rocks is represented as follows:

SiO_2	Al_2O_3	Fe_2O_3	MgO	CaO	Na_2O	K_2O	Loss	Total
71.60	13.60	2.40	.21	2.30	5.55	3.53	.70	= 99.89

As the rock approaches the gabbro it becomes less acid and the proportion of augite in it increases. This is the lower portion of the mass as it was originally intruded. Its more basic nature as compared with the rest of the rock is explained as due to the absorption of parts of the gabbro with which the granophyre is in contact.

The same author[6] also records the existence of a greisen, which is a phase of the well known Skiddau granite. The greisen consists essentially of quartz and muscovite, but remnants of orthoclose are still to be detected in it. The mica is regarded as having been derived largely from the feldspar.

Sheet and Neck Basalts in the Lausitz.—The basalts of the neighborhood of Seifeirnersdorf and Warnsdorf in the Lausitz, Saxony, occurs in sheets according to Hazard,[7] and in volcanic rocks. The sheet rocks are nepheline basalts, nepheline basanites and feldspathic glass basalts. The neck forms are hornblende basalts, sometimes with and sometimes without nepheline. The constituents of all are magnetite, apatite, augite, biotite, nepheline and glass in varying quantities, with feldspar, olivine and hornblende in different phases. Sometimes the mineral nepheline is absent, but this happens mainly in the glassy varieties, where its components are to be found in the glassy base. There are intermediate varieties between the hornblende and the oli-

[5] Quart. Journ. Geol. Soc., 1895, p. 125.
[6] Ibid, p. 139.
[7] Min. u. Petrog. Mitth., XIV, p. 297.

vine basalts corresponding to geological masses intermediate in characteristics between volcanic sheets and necks. In many of the neck rocks the hornblende is seen to have been partially resorbed and changed to augite. The continuation of the resorbtive process until every trace of the hornblende was dissolved, may account for the absence of the mineral in the sheet rocks.

Petrographical Notes.—In an article whose aim is to call forth more accurate determinations of the feldspars in volcanic rocks, and one which gives a practical method for making this determination, Fouqué[8] has described briefly the volcanic rocks of the Upper Auvergne, the acid volcanics of the Isle of Milo and the most important rocks in the Peleponeses and in Santorin. Among the varieties described are doleritic basalts, andesitic basalts, labradorites, andesites, obsidians, trachyte andesites, phonolites, andesitic diabases, rhyolites, dacites and normal basalts. The labradorites are composed largely of microlites of labradorite with a few augites and tiny crystals of olivine in an altered glassy base. In all these cases the author has shown that the rocks contain several different feldspars at the same time, and in each case he has determined their nature. The method made use of in the determination is based on the observation of extinction angles in plates cut perpendicular to the bisectrices.

In a well written article on complementary rocks and radial dykes Pirsson[9] suggests the name of oxyphyre for the acid complementary rock, corresponding to the term lamprophyre for the basic forms. He also calls attention to the fact that the dykes radiating from eruptive centers are usually filled with younger material than that which composes the core at the center. The dykes cutting the central mass will generally be oxyphyres and the more distant ones lamprophyres.

Cordierite gneisses are reported by Katzer[10] from Deutshbrod and Humpolitz in Bohemia, where they are intruded by granite veins, and where masses of them are occasionally completely surrounded by granitic material.

In the examination of a large series of granites and gneisses from the borders of the White Sea, Federow[11] discovered that garnet is present in large quantities when plagioclase is absent and vice versa.

In a general article on the Catoctin belt in Maryland and Virginia,

[8] Bull. Soc. Franc. d. Min., XVII, p. 429.
[9] Amer. Journ. Sci., 1895, p. 116.
[10] Min. u. Petrog. Mitth., XIV, p. 483.
[11] Ibid, p. 550.

Keith[12] gives very brief descriptions of the granites, quartz porphyries, andesites and the Catoctin schist of the region. The last named rock is apparently a sheared basic volcanic. All the rocks present evidence of having suffered pressure metamorphism.

GEOLOGY AND PALEONTOLOGY.

Notes on the Fossil Mammalia of Europe.—I, COMPARISON OF THE AMERICAN AND EUROPEAN FORMS OF HYRACOTHERIUM.—Historically speaking Hyracotherium is one of the oldest of known fossil Perissodactyla, and it is of importance phylogenetically to compare the representatives of this genus in Europe with those of America, in order to acquire an exact knowledge as to the evolution of the molar cusps of the New and Old World species. My attention was called to this subject on account of having studied *Euprotogonia* of the Puerco, a genus which as well known, is considered to have one of the most primitive types of Ungulate molars.

The importance of having accurate drawings of the teeth of fossil mammals is nowhere better illustrated than in *Hyracotherium*. In the case of the enlarged drawing of the teeth[1] of *H.* (=*Pliolophus*) *vulpiceps* which has been copied extensively in works on vertebrate palæontology, we obtain quite an erroneous idea of the exact form of the molar cusps.

Kowalevsky[2] in his great work on "*Anthracotherium*" figures some of the molars of the type of *Hyracotherium* namely: *H. leporinum*, and I should judge from his description that he had studied Owen's type in London. However, his criticism of Owen's drawing of the type of *Hyracotherium* is very accurate, and as Kowalevsky remarks, Owen's figures gives one the idea that the teeth of the type are strictly bunodont, whereas they are really transitional in structure between a real bunodont type, such as *Euprotogonia* and a truly lophodont form like *Systemodon*.

[12] 14th Ann. Rep. U. S. Geol. Survey, p. 285.

[1] Jour. of the Geog. Soc. of London, 1858, p. 54.

[2] Monographie der Gattung Anthracotherium, p. 205.

On my recent visit to London[3] I took the opportunity to examine both of the types of *Hyracotherium* (*H. leporinum* and *H.* (=*Pliolophus*) *vulpiceps*). In general the external cusps of the superior molars in both forms is lenticular in section, being considerably drawn out anteroposteriorly, and the intermediate tubercles are well extended transversely. In the drawing of the type specimen of *H* (=*P.*) *vulpiceps* the molars are represented as greatly enlarged, and their internal portions shown as complete. In the original specimen the teeth are damaged internally, and it is with some diffiulty that the form of the cusps can be made out. However, I am satisfied that the internal cusps are not really bunodont as shown in the plate, but like the American forms of *Hyracotherium* these cusps are extended transversely and form by wear slight ridges with the intermediate tubercles. In comparing the upper molars of both types of *Hyracotherium* of England with those of the Wasatch of America, I find them to be in exactly the same stage of evolution as to the form of the cusps.

Lydekker[4] speaks of the posterior transverse crest of the upper molars in the type of *H.* (=*P.*) *vulpiceps* as not being represented as sufficently well developed in the plate, but this crest on the last molar is drawn correctly, and on the other two molars it is as nearly as well developed. A comparison of the upper molars in both types of *Hyracotherium* with those of *Euprotogonia*, reveals the fact that the form of the cusps in the former genus has undergone a progressive change; and this is seen especially on the last upper molar which is quadritubercular with a large development of the metaloph, whereas in *Euprotogonia* the last upper molar is tritubercular. Again the third upper premolar in the English types of *Hyracotherium* is tritubercular whereas in *Euprotogonia*, this tooth has only one external cusp.

The species of *H.* (=*Pliolophus*) *vulpiceps* is of importance, as in this specimen we have both upper and lower teeth belonging to the same individual. I should like to particularly emphasize the point that the last lower premolar in the type of *Pliolophus* is *simpler in structure than the first true molar, the posterointernal cusp being absent.* This character distinguishes this type from some forms of the American Wasatch which have been referred to *Pachynolophus*.

[3] I wish to express here my thanks for the privileges I enjoyed in examining specimens in the British Natural History Museum, and especially to thank Sir W. H. Flower for his kindness. I am also indebted to him for having been able to visit the Royal College of Surgeons. Mr. C. W. Andrews of the Geological Department. I am also much indebted for his many courtesies.

[4] Catalogue of Fossil Mammals in British Museum. p. 11.

The question now arises what is *Pachynolophus*, or in other words what is the exact generic definition of this genus, and does it really occur in the Wasatch of America. It is usually stated that *Pachynolophus* is separated from *Hyracotherium* by the fact, that the last premolar is molariform but as far as I have been able to investigate, this definition will apply to only one European species, namely: *Pachynolophus* (=*Hyracotherium*) *siderolithicus*, and even in this species the last upper premolar exhibits a good deal of variation in its structure. Rütimeyer[5] figures Pictet's species, *P. siderolithicus*, which in this specimen has the last upper premolar molariform, but in the same plate (fig. 21) is given another last upper premolar, which Rütimeyer referred to the same species. This tooth is tritubercular, or simpler in structure than the true molars.

Among the specimens of *Pachynolophus* in the collection of the Jardin des Plantes, Paris, there is a series of loose teeth from the Siderolithic du Mauremont, which are of considerable interest as they were studied by Kowalevsky, and referred by him to *P. siderolithicus*. This series contains at least one last upper premolar, and it has exactly the same character as that figured by Rütimeyer, in other words this is another case in which this tooth in *P. siderolithicus* is simple in structure. In *Pachynolophus duvalii* as figured by Rütimeyer, the last upper premolar is tritubercular, and this tooth has the same structure in *P. desmarestii*. In *Pachynolophus cessarasicus* figured by Filhol[6] from the Middle Eocene of Céssai, the last upper premolar has only one internal cone and the two transverse ridges diverging from it are well developed. The length of the skull in this species is about one-third greater than that of *Hyracotherium venticolum* of the Wasatch.

Numerous species of *Pachynolophus* occur in the different horizons of the Eocene of France, but in nearly all cases, they are represented either by upper or lower molars which were not found together. I believe the English *Hyracotherium leporinum* is the only known form in Europe in which both upper and lower molars were found associated, and actually belong to the same individual.

As is well known it is the last lower premolar which first becomes molariform, consequently if we find forms in which this tooth is simpler in structure than the molars, we can safely conclude that none of the superior premolars are molariform. In a jaw referred to *Pachynolophus* in the collection of the Jardin des Plantes from Phosphorites, containing all the lower premolars, the last tooth of this series is not molari-

[5] Eocäne Sangethiere Welt von Egerkingen, pl. III, figs. 18–21.

[6] Vertébres Fossiles d'Issel Mém. Soc. Geog. de France, 1888, pl. XX, fig. 13.

form. As the Phosphorites are considered to represent the top of the Eocene, we should certainly expect to find in any species of *Pachynolophus* from this formation the last premolar as complex in structure as the molars. In this jaw, however, the last premolar is not molariform and the collection contains a crushed skull of the same species of *Pachynolophus* in which all the superior premolars are simpler in structure than the true molars.

I can find no good generic differences based on tooth structure separating *Propalæotherium* from *Pachynolophus*, and shall consider the former genus as a synonym of the latter in this paper. In *Propalæotherium* the species are much larger than in those of *Pachynolophus*, but certainly size alone can not be considered as of value in generic definitions. In all of the species included in *Propalæotherium*, the premolars are simpler in structure than the true molars.

I believe, however, if we divide the various known species of the *Hyracotheriinæ* into genera according to the complication of the premolars, that we shall be adopting an artificial character, and as shown above. Of all the known forms of *Pachynolophus*, *P. siderolithicus* is the only one in which the last upper premolars is molariform and even this species shows considerable variation in this respect. I conclude then that the only natural classification of these forms is a careful analysis of the form of the molar cusps, and to group the species into genera according to the development of the same; I refer here especially to the European forms of *Hyracotherium* and *Pachynolophus*.

Having thus attempted to show that in nearly all the European species of *Pachynolophus* the last premolar is simpler in structure than any of the true molars, I come to consider what are the generic differences separating *Hyracotherium* from *Pachynolophus*. Kowalevsky studied *Pachynolophus siderolithicus*, and if we compare the molars of this species with those of *Hyracotherium angustidens* from the Wasatch of America, we observe at once that the external cusp of the upper molars in the latter species are nearly round in section, and they are scarcely at all flattened. There is no mesostyle and the height of the crown is very low or strongly brachydont. In *P. siderolithicus* the ectoloph is considerably lengthened from above downwards, and the external cusps are strongly flattened, with a prominent mesostyle. In all the species of *Pachynolophus* which I have studied the molar crowns are higher than those of *Hyracotherium*, and in all the mesostyle is strongly developed. The latter characters demonstrate that the molars of *Pachynolophus* have reached a higher stage of evolution than those of *Hyracotherium*, and this transformation in the form of the molar cusps

points to a still higher differentiation so characteristic of the later genera of the Equine series. The characters above enumerated as distinguishing the molars of *Pachynolophus* from those of *Hyracotherium*, I think should be considered as generic. At least they are the only valid ones which I can discover at present.

Species of Perissodactyle Ungulatus allied to *Hyracotherium* occur in the Wasatch of America, which have been referred to *Pachynolophus*. In these forms the last inferior premolar is truly molariform, but the superior molars are of the primitive type namely; with low crowns and nearly bunodont external cusps. As this a combination of characters, which as far as known does not occur in the true *Pachynolophus* of Europe, I think accordingly that these species should be placed in a different genus from *Pachynolophus*. Prof. E. D. Cope has shown that the generic name *Orotherium* Marsh, has been anticipated by *Orotherium* Aymard, consequently I believe that the name *Orohippus* should be reinstated, and applied to those species of *Hyracotherium* which have the last lower premolar truly molariform in structure. I have already stated that in the type specimen of *Hyracotherium* (=*Pliolophus*) *vulpiceps* the last lower premolar is simpler in structure than in any of the true molars. Accordingly those species with the complex lower premolar represent a true generic stage, and as is already shown they differ from the true *Pachynolophus*.—CHARLES EARLE, *Laboratoire de Palæontologie Jardin des Plantes, Paris, Dec.* 20, 1895.

The Glossopteris Flora in Argentina.—A collection of fossil plants from Bajo de Velis, a league from the entrance to the Cantana valley in Argentina, has enabled Dr. F. Kurtz to establish the age of the fossiliferous shales of that region. The author gives tabulated comparisons of the flora of the Bajo de Velis beds with similar floras found at the Cape of Good Hope (Ekka-Kimberly beds), in peninsular India (Kaharbari beds), in New South Wales (Newcastle beds) and in Tasmania (Mersey Coalfield). From these tables it is seen than the specimens found at Bajo de Velis are most nearly related to those of the lower Gondwanas of India. Dr. Kurtz accordingly corrrelates the fossiliferous shales of Bajo de Velis with the lower Gondwanas of India, and agrees with the paleophytologist, O. Feistmantel, in assigning these beds to the Permian age.

Up to date but three rock formations in Argentina have yielded fossil plants: Retamito in San Juan, which has been shown by Dr. Szajnocha to be Lower Carboniferous; Bajo de Velis which is Permian; and

a series of beds in Mendoza, San Juan and La Rioja determined by Prof. Geinitz as Rhætic.

These data are commented on by the Director of the Geological Survey of India to the effect that one of the chief points of interest in connection with the discovery of Gondwana plants in Argentina lies in the fact that we have an unquestionable lower carboniferous series (Retamito) in the neighborhood of which (and probably unconformably to it) a series of beds is found, which contains well known Lower Gondwana species of plants, thereby limiting the geological range of the lowest beds of it, at all events to upper Carboniferous at most, which is a further confirmation of the views generally adopted by the Geological Survey of India. The genus Glossopteris proper is wanting, but the other genera characteristic of that flora are present. (Rec. Geol. Surv. Ind., Vol. XXVIII, 1895.)

Geological News.—PALEOZOIC.—From a petrographic study of the igneous rocks near St. John, N. B., Mr. W. D. Matthew classifies the Pre-Cambrian of that region as follows: A. Laurentian composed of (1) Portland group and (2) Intrusive granite, B. Huronian composed of (3) Coldbrook group, of volcanic rocks, (4) Coastal group, of volcanic and sedimentary rocks, (5) Etcheminian or Basal Series, of sedimentary rocks, and (6) Kingston group, of metamorphosed volcanics. (New York Acad. Sci. XIV, 1895.)

MESOZOIC.—In studying the fossils obtained by M. Gautier from Madagascar, M. Boule comes to the conclusion that the Jurassic deposits of eastern Africa and those of the western slopes of Madagascar appear to have been laid down in a great interior sea, an Ethiopian Mediterranean, which was separated from the Pacific by an Indo-Madagascar peninsula.

Furthermore, that during the Upper Cretaceous there was land communication between the African continent, Madagascar and Hindustan. (Bull. Mus. d'Hist. Nat. Paris, 1895.)

According to R. W. Ells, the whole range of North Mountain, which cuts off the valleys of Cornwallis and Annapolis rivers from the Bay of Fundy, is an overflow of igneus rock which has issued through a line of fissure transversing the red Triassic beds, and is, therefore more recent than the latter. At several places the trap is overlaid by newer sedimentary beds of limestones and shales. No fossils have as yet been found in these sedimentary strata. The author calls attention to their importance and the desirability of a thorough exploration in order to determine their age since they represent the highest group of stratified sedimentary rocks in Eastern Canada. (Trans. Nova Scotian Inst. Sci. Halifax, Vol. I, 1894, p. 416.)

Two new species of Fishes from the Rolling Downs Formation (Lower Cretaceous) of Queensland are described by A. S. Woodward. They represent species of the genera Portheus and Cladocyclus, to which he gives the names *australis* and *sweetii* respectively. This discovery of these fossils is of considerable interest, since with the exception of a few Selachian teeth and vertebræ, and a fine species of Belonostomus, no cretaceous ichthyolites of importance have hitherto been described from this colony. (Ann. Mag. Nat. Hist. (6) XIV, 1894, p. 444.)

CENOZOIC.—Fossil Ants are reported from the Bembridge limestone (Eocene) of the Isle of Wight. They are referred by P. B. Brodie to the genera Formica, Myrmica and Camponotus, and some others not yet described. The first two genera have also been found in the Baltic Amber. (Nature, 1895, p. 570.)

The Champlain epoch is correlated by Prof. Hitchcock with the Mecklenburg stage of Geikie. Both have the characteristic marine mollusca fauna, the Arctic flora (*Yoldia* beds of the Baltic) and best illustrate the isobases of De Geer. (Bull. Geol. Soc. Amer., Vol. 7, 1895.)

VEGETABLE PHYSIOLOGY.[1]

Smut Fungi by Oscar Brefeld.—At last we have in two big quartos, with numerous plates, the long promised volumes on the smut fungi. The work which is here completed was begun more than 12 years ago. The earlier experiments were gathered together and published in 1883 in a volume of 220 pages with numerous plates under the title of *Die Brandpilze I*, forming Heft V of Dr. Brefeld's Untersuchungen, the most important and revolutionary portion of this volume being the demonstration that the smut fungi, a goodly number at least and presumably all of them, although previously supposed to be strictly parasitic were capable of growing saprophytically and of multiplying indefinitely in dung in the form of sprout conidia, closely resembling yeasts, if not identical with many forms previously referred to this group. Some years later in an address before the agricultural club of Berlin, Dr. Brefeld communicated the most important results of his

[1] This department is edited by Erwin F. Smith, Department of Agriculture, Washington, D. C.

magnificent infection experiments, but now for the first time we have full details of all the laboratory and field investigations. In the limits of this review it will be possible to notice only the first of these two volumes. This forms Heft XI of the Untersuchungen and is entitled *Die Brandpilze II.* It deals principally with infection experiments and gives in full the results obtained with *Ustilago Carbo* on oats, *U. cruenta* on sorghum, and *U. maydis* on maize. These experiments were carried on through a period of four years with striking results and in case of corn, with most unexpected ones. Space forbids entering into much detail. Those who wish for details will naturally consult the volume itself. Suffice to say that the infective material consisted of the yeast-like conidia propagated in nutrient solutions made from fresh horse dung.

In case of oats the best results from direct infection were 17 to 20 per cent. of smutty plants, obtained by spraying during the earliest stage of germination. Infections made when the embryo was one cm. long gave only 7 to 10 per cent of smutty plants; when it was 2 cm. long (500 plants), only 2 per cent became smutty. When the plumule had pushed through the enfolding sheath scarcely any of the plants could be infected, 200 seedlings in this stage yielding only 1 per cent of smutty plants and 200 more remaining entirely free. The infections took place through the young axis and also through the sheathing leaf so that both Wolff and Kühn were right, but a majority of the infections were through the young axis. In a second series of experiments garden earth was sprayed with the smut conidia and two days later oats were planted 1 cm. deep and subsequently transplanted to the open field: 300 of these seedlings yielded 5 per cent of smutty plants, and 300 more, 4 per cent., i. e. a much smaller per cent than was anticipated. In a third series fresh horse dung was mixed with garden earth which was then abundantly impregnated with the smut conidia. Three days later oats which had been soaked but were not yet germinated were planted in this soil at a depth of scarcely 1 cm. These seedlings were divided into two lots, 300 were kept for a time in the laboratory at a temperature of over 15°C., and 300 were placed in the cellar where the temperature did not exceed 7°C. Of the 300 kept in the laboratory 27 to 30 per cent finally became smutty; of those kept in the cellar, where germination proceeded more slowly, 40 to 46 per cent became smutty. This shows clearly that fresh horse dung greatly favors the development of smut and that weather which retards germination is also favorable. In the fourth series of experiments the infectious material was derived from conidia cultivated for a long time arti-

ficially, a few of the spores being transferred to a fresh nutrient solution every four days. The first trial (500 seedlings) was with conidia which had been cultivated in this manner for six months. These seedlings yielded from 7 to 10 per cent of smutty plants. The second trial was with conidia which had been cultivated for a year. This experiment was almost wholly negative, 300 of the seedlings yielded no smutty plants and 200 more gave only 1 per cent. The explanation was not far to seek since at the end of this period the conidia had almost wholly lost the ability to send out germtubes and along with it the power to infect the plants. Microscopic examination showed that the germtubes can penetrate into any part of the young seedling but this does not necessarily mean infection. The latter takes place only when the smut hyphæ are able to reach that part of the plant where the smut beds form. In all of these experiments the smut germs penetrated the young seedlings but the smut beds appeared only in the floral organs, some months intervening between the entrance of the fungus and the appearance of the smut in a totally different part of the plant. Those germtubes which enter the plant and fail to reach the incipient ovaries become enclosed in the mature tissues of the host plant and are incapable of further growth and this frequently occurs even in young seedlings.

The infections obtained with the big sorghum plant are even more interesting. Nearly all of the first series of infections were destroyed by a hail storm, but of the 32 plants which escaped 12 became smutty. The seedlings of the second series were infected indoor in March and set out the first of May. The plants grew luxuriantly and by the middle of August had reached a height of 5 to 7 feet. The first smutty panicle appeared August 16 and for some time thereafter it appeared as if all of the plants would be smutty, the infected panicles developing first. Finally sound ones began to appear. In the end there were 158 smutty plants out of 274. A third series of experiments was instituted to determine in what stage of germination the sorghum plant is most susceptible: 252 seedlings sprayed in the earliest stage of germination, gave 180 smutty plants. "The development of the smut in the earliest and strongest plants, which reached a height of 8 feet, was striking. The big panicles were attacked in toto and projected out of the luxuriant green foliage like black brooms." There can be no doubt that infection stimulates the growth of the plant. Older seedlings yielded less striking results: 150 which were infected when the embryo was a centimeter long, gave only 24 smutty plants; 190 infected when the embryo was 1½ cm. long gave 12 smutty panicles; 221 infec-

ted when the plumule had begun to push through the sheath gave 5 smutty plants; finally, 150 infected when the plumule had pushed through the sheath about 1 cm., remained entirely free from smut. Microscopic examinations made a few days after the conidia were sprayed on the seedlings showed that germtube penetrations were very common in that experiment which yielded over 70 per cent of smutty plants, infrequent in those which yielded only a small per cent of smutty plants, and altogether absent in the plants which remained entirely free from smut. As in oats, the smut was confined exclusively to the panicle, and the bulk of the infections took place during the earliest stage of germination, the tissues of the growing seedling very soon becoming immune.

The results with maize were very surprising since they developed three wholly unexpected facts, viz.: (1) The germtubes are capable of penetrating any young rapidly growing part of the plant; (2) The growth of the fungous hypha which has gained entrance into the plant is narrowly localized, the sporebeds developing in situ; (3) There is no period of rest, the smut beds developing immediately, i. e. within two or three weeks of the date of infection. Previous to these experiments it was supposed that corn smut entered the plant when it was a seedling and followed the same law of development as oat smut. In the first series of experiments, which proceeded upon this supposition, the smut conidia were sprayed upon 200 seedlings in the earliest stage of germination; upon 100 which were a little older; upon 100 still further advanced; and, finally, upon 100 when the plumule was pushing through the sheath. This work was done in the laboratory and after 14 days the plants were set out in the garden. Contrary to all expectation, very few penetrations could be found even by the most careful microscopic examinations, and these were confined to the root node, none being found upon the sheath,—everywhere over the surface crept the germtubes without being able to enter. These plants were under daily observation and after 10 to 14 days a few lagged behind the rest in growth, and on being pulled up smut pustules were found on the axis a little above the root node. Of the whole 500 seedlings, only a few became smutty, viz., 4 per cent in the youngest and 1 to 2 per cent in the older seedlings. In all of them the smut pustules appeared on exactly the spot where the germtubes had entered the plant and within three weeks of the date of infection. All the other plants grew to maturity and remained free from smut. Similar results were obtained from an experiment in which soaked, ungerminated kernels of corn were planted in a dunged soil which had been

abundantly infected with smut conidia. Of the 50 plants thus treated one died at the end of 4 weeks from a smut pustule on the axis, and the rest developed without any appearance of smut. Another experiment was undertaken with 150 seedlings still further advanced, the conidia being sprayed upon them, but this also gave negative results. No germtube penetrations could be found and no smut appeared upon any of the plants. These results led to a good deal of speculation and finally to the following experiments: The first of these was with plants a foot high, having a well developed cornucopia-like summit formed by the closely wrapped bases of the large outer leaves. One hundred plants were selected and into these cornucopias a nutrient solution containing smut conidia was injected. They were covered with straw matting five days to keep off rain and then freely exposed. On the tenth day, as growth continued and the infected parts were pushed up into sight, there was a changed appearance. The parts of the leaves touched by the infectious fluid were paler than the upper noninfected parts and suggested chlorosis. This appearance was visible in different degrees on all the infected plants? Already there were slight appearances of pustules and within a day or two they became very distinct, finally covering the whole infected surface with a smutty crust. Scarcely one of the male inflorescences escaped and the axis between the leaves was also smutty in so far as the infective material could reach it. Not one of the hundred plants escaped infection, the youngest suffering most. For the next experiment younger plants were selected, i. e. those about six inches inches high. In many of these the cornucopia was not well developed and allowed the infectious fluid to run out and waste and the infection miscarried. All, however, that were large enough to retain the conidia were killed outright by the development of smut pustules, the plants twisting and curving in all sorts of shapes and frequently wilting before the smut spores were mature. The third experiment was with plants 1½ feet high. Here the cornucopias were wide open and took in large quantities of the infectious fluid, which penetrated deep into the heart of the plant. After three weeks the male inflorescences appeared, but in only six plants out of 50 could any symptoms of smut be found and upon these the pustules were small and scattering. On the leaves there were wrinkled, white spots which, however, did not develop into smut pustules but subsequently became green and nearly normal in appearance. Scattered smut pustules were found on the axis at the base of the internodes in 7 cases, and the effect of the fungus was also visible on some of the upper blossoms which remained white and dried up without developing. Aside from these scattering

symptoms all of the plants remained sound, ripening normal ears. The fourth experiment, with still larger plants, gave wholly negative results. The heart of the plant proved immune, and normal ears developed. In another experiment female inflorescences were infected as soon as there was any indication of a forming ear, the Nahrlösung containing the conidia being injected into the narrow opening between the ligule and the axis. Smut pustules appeared in great numbers within 18 days but only on the parts which were actually reached by the injected fluid. Another experiment was made when the ears were in blossom. All the kernels became smutty and single ears reached the size of a child's head. In another experiment varying amounts of the lower part of the ear were protected from the fungous spray by wrapping them in blotting paper. In this case only the exposed kernels became smutty, showing again conclusively that the infection is purely local. The silk though much exposed to the conidial spray showed not the least trace of injury, having passed out of the meristematic stage. In still another experiment the kernels of the ear were sprayed with the smut conidia when they were more than ½ grown. The result was wholly negative; no smut appeared. Another experiment showed that the adventive aerial roots can also be infected if sprayed in an early stage of their growth. In short, any meristematic part of the maize plant is liable to direct infection and this is made easy by the fact, which is also Dr. Brefeld's discovery, that the corn smut fungus, unlike that of oats and sorghum, is richly provided with *aerial* conidia, which are easily carried or blown from the soil to any part of the plant. The consequent desirability of keeping the soil of corn fields free from smut spores, by removing and burning all smut pustules before they have ripened and shed, must be apparent to all. The corn smut spores seldom germinate in water, as is well known, and infection of the plant probably takes place only when the latter have an opportunity to germinate in the soil and produce the aerial conidia, this germination in the soil being greatly favored by the presence of dung. The volume contains VI, 98 pages of text and 5 lithographic plates, mostly colored.—Erwin F. Smith.

ZOOLOGY.

The Paroccipital of the Squamata and the Affinities of the Mosasauridae once more. A rejoinder to Professor E. D. Cope.—I. *The paroccipital.*—In 1870, Cope[1] designated the occipital externe, Cuvier, paroccipital, Owen with Huxley's name opisthotic, and homologized it with the squamosal of the Lacertilia and Ophidia. This opinion is held up in 1894 and in September, 1895,[2] but for the name opisthotic the name paroccipital is then used. On the other side, it is admitted by everybody else that the paroccipital, Owen (opisthotic, Huxley), which is free in the Testudines, is united with the exoccipital in the Lacertilia; the posterior portion of this bone, which is visible from behind, has been called the paroccipital process; in its anterior portion where it reaches the basioccipital it contains the posterior semicircular canals. I have stated in my last note (AM. NAT., Nov., 1895) that in young Sphenodons the paroccipital is free from the exoccipital exactly as in the Testudines and that Siebenrock has proved without question that the outer portion of the exoccipital of the Lacertilia, which lodges anteriorly the posterior semicircular canals, represents the same element. The paroccipital process of the exoccipital in Sphenodon is, of course, identical with the paroccipital process in the Lacertilia.

To this, Prof. Cope replies: "Baur asserts that the socalled parotic process [I said paroccipital process] of the exoccipital which supports the quadrate in the Squamata is the same element as that termed opisthotic by Huxley. This I deny, and believe that in this it is Baur and not myself who has fallen into error. *Siebenrock, instead of asserting this to be the case, denies it in the following language:*† 'It is not the processus paroticus of the pleuroccipital (exoccipital) which is homologous with the (paroccipital, Owen), opisthothic Huxley, but the portion anterior to the foramen nervi hypoglossi superius which protects the organ of hearing.' Siebenrock here uses the names of Owen and Huxley as refering to the same element, *but he makes the clear*

[1] Cope, E. D. On the Homologies of the Opisthotic Bone, Amer. Asso. Adv. Sc., XIX.

[2] Cope, E. D. On the Homologies of the posterior cranial arches in the Reptilia, Trans. Am. Philos. Soc., Vol. XVII, Apr. 27, 1892; also Am. Nat., May, 1892. The Osteology of the Lacertilia, Proc. Am. Philos. Soc., Vol. XXX, May 10, 1892, pp. 185–211. Amer. Nat., Sept. 1895, p. 855–856.

† Italics are mine.

distinction which is the important point, between the parotic process of the exoccipital and the element which contains the posterior semicircular canal.† What then is the element which articulates with the quadrate in the different orders of the Reptilia?"

The sentence quoted from Siebenrock is misleading. Siebenrock *does not distinguish between the parotic process of the exoccipital and the element which contains the posterior semicircular canal.* He says: *not only the parotic process but the whole portion anterior to the foramen is homologous to the paroccipital.* This whole portion, of course, contains also the parotic process. The sentence of Siebenrock translated by Cope is printed at the end of the paper in a résumé. A full account of the conditions is given on p. 209. "Die bisherige Anschauung, dass am Processus paroticus des Pleuroccipitale (exoccipitale) das Opisthoticum zu finden sei, ist daher absolut unrichtig, *sondern der ganze vordere Theil des Pleuroccipitale, welche die hintere Partie des Gehœres enthält, sammt dem Processus paroticus ist als das eigentliche Paroccipitale aufzufassen.*† Vergleicht man dasselbe mit dem bei den Schildkröten zeitlebens separirten Paroccipitale, so ergiebt sich schon aus der Lage und Function die Homologie der beiden Knochen." And later: "Die gleichen Verhältnisse bestehen bei Hatteria, nur bleibt hei dersellben das Paroccipitale viel laenger vom Pleuroccipitale (exoccipitale) getrennt, als bei Lacerta."

That Prof. Cope has not studied Siebenrock's paper is also evident from the following sentence: "In the Testudinata, and according to Baur, in Sphenodon, the element which extends externally from the exoccipital to the quadrate is continuous with the opisthotic, but the semicircular canal is included in its proximal part only. Here the structure is entirely different from that which characterizes the Squamata, where the opisthotic does not extend distal of the canal and fuses early with the exoccipital." It is still more evident from the following words: "In the Squamata, where the opisthotic is restricted to the region of the canal and does not reach the quadrate, this socalled paroccipital is distinct." Cope thinks the paroccipital + otic portion of the paroccipital or opisthotic in the Testudines is not homologue to the paroccipital + otic portion of the paroccipital or opisthotic of the Squamata, and has the idea that this bone, paroccipital, Owen, opisthotic, Huxley, occipitale externe, Cuvier, consists of two elements, the outer one—the paroccipital—and the auditory portion, the opisthotic. He admits that "the direct evidence for such a primitive division of this element (occipital externe, Cuvier; paroccipital, Owen; opisthotic, Huxley)

† Italics are mine.

PLATE IV.

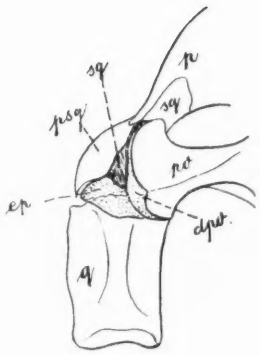


FIG. 1

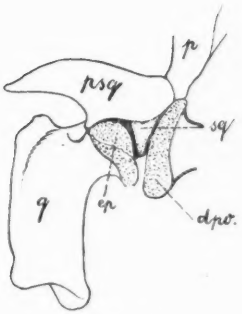


FIG. 2.

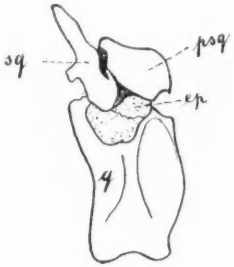


FIG. 3.

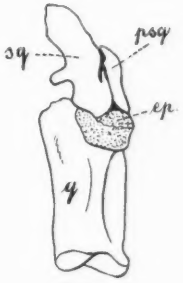


FIG. 4.

Conolophus subcristatus Gray. Quadrate and connections.

in the Testudinata has, however, yet to be produced, and I am entirely willing to give up the view above defended, should it turn out on further investigation to be untenable."

There is no further investigation necessary. The bone in question is a single element, as is shown, not only by comparative anatomy, but also by embryology. This element always is free in the Testudines; it is free in the young Sphenodon; and it is united with the exoccipital in the Squamata. There is not the slightest difficulty in this question.

One word about the squamosal. The squamosal of the Lacertilia and Ophidia is connected with the parietals and stands on the quadrate, outside of this element we have in the Lacertilia with well developed postorbital arch another element, which originally is united with the postorbital and is also connected with the squamosal and quadrate. This bone is the *prosquamosal*.[In Sphenodon the squamosal and prosquamosal are united, but in the Jurassic Saphæosaurus (Sauranodon) these two elements are free as in the Lacertilia. In the Testudines the squamosal represents the squamosal of Sphenodon, i. e., the squamosal + prosquamosal of Saphæosaurus and the Lacertilia. Prof. Cope says: "the squamosal of the Squamata is homologous to the paroccipital (opisthotic, Huxley, occipital externe, Cuvier) of the Testudines. This is impossible, since the paroccipital of the Testudines is the homologue of the paroccipital process of the Lacertilia, which in front contains, exactly as in the Testudines, the posterior semicircular canals. In the Mosasauridæ we have the same conditions as in the generalized Lacertilia. The paroccipital and exoccipital are united; connected with the quadrate we find two elements—the inner one connected by its upper branch with the parietal process; the outer one with the postorbital. These bones are, of course, homologous to the squamosal and prosquamosal of the Lacertilia.

II. *The Affinities of the Mosasauridæ.*—Cope maintains, contrary to my statement, "that in all Lacertilia the exoccipital supports the quadrate, and that in the Pythonomorpha and the Ophidia the exoccipital does not support it or generally touch it." He also maintains "that the paroccipital (squamosal, Baur) does support the quadrate in the Ophidia, while it is only in contact with a very small part of it in the Lacertilia." I have denied in my last note that in all the Lacertilia the exoccipital supports the quadrate, and I repeat it here.

I have before me disarticulated and complete skulls of Iguana, Ctenosaura, Amblyrhynchus and Conolophus. In none of these I find an articular facet on the paroccipital (exoccipital Cope), for the

quadrate. The paroccipital even does not touch the quadrate, but is connected by the anterior and upper portion of its distal process with the inner side of the squamosal; the face of the distal end of the paroccipital is entirely free from any connection and is always visible from the outside. The paroccipital process is placed behind and also above the upper face of the quadrate for these elements. In none of the genera mentioned above I find a face on the paroccipital for the quadrate, but a face for the squamosal. In the Mosasauridæ I find the same. The quadrate is supported by the squamosal and the squamosal is connected by its inner process with the anterior face of the distal end of the paroccipital; the prosquamosal takes also part in the support of the quadrate. We have, therefore, the same conditions as in the genera mentioned. The statement that the Mosasauridæ agree with the Ophidia in the relations of the quadrate, is absolutely incorrect.—G. Baur, University of Chicago.

Explanation of Figures.

Fig. 1.—*Conolophus subcristatus* Gray. Left quadrate and its relations to the squamosal, prosquamosal and paroccipital, from behind.

Fig. 2.—*Conolophus subcristatus* Gray. Left quadrate and its relations to the squamosal, prosquamosal and paroccipital, from outside and little behind.

Fig. 3.—*Conolophus subcristatus* Gray. Right quadrate and its relations to the squamosal and prosquamosal, from behind and a little inside.

Fig. 4.—*Conolophus subcristatus* Gray. Right quadrate and its relations to the squamosal and prosquamosal, from behind.

q=Quadrate.

ep=Epiphysis of quadrate.

sq=Squamosal (mastoidien, Cuvier; opisthotic, Cope, 1870; paroccipital, Cope, 1892–95).

psq=Prosquamosal (temporal, Cuvier; squamosal Cope, 1870; supratemporal, Cope, 1892–95). (Baur, G. Anat. Anz. Bd., X, p. 327.)

p=lateral process of parietals.

po=paroccipital (exoccipital, Cope).

dpo=distal end of paroccipital.

Addition.—After the manuscript of this note had been sent to the editor, I received the November number of the Annals and Magazine

of Natural History, containing a communication of Mr. G. A. Boulenger; "Remarks on the value of certain cranial characters employed by Prof. Cope for distinguishing Lizards from Snakes." Boulenger shows also that Cope's statement, in regard to the relations of the squamosum to the quadrate of the Lacertilia, is quite incorrect.

Criticism of Dr. Baur's rejoinder on the homologies of the paroccipital bone, etc.—I. *The Paroccipital Bone.*—It seems that I have not yet made clear to Dr. Baur my position as to this element in the Reptilia. The ground for it is paleontological, and when Dr. Baur considers the question from this standpoint, he will probably find some of his very positive assumptions not proveable at present.

In the first place, we agree as to the identity in the Lacertilia, Pythonomorpha and Ophidia of the elemeut which he calls squamosal, and which I call paroccipital. Whatever be the place of this element in the Mammalian skull, it has certainly not been proven to be the squamosal, hence I object to the name which Dr. Baur uses for it, in which position I agree with various authors. It remains to be seen whether the term paroccipital, which I have hitherto used, be appropriate. I must here repeat that at no time since 1871 have I confounded it with the opisthotic of Huxley, not even in those cases (as Testudinata) where I have supposed the two elements to be fused together.

Now the characters of this paroccipital in the Pythonomorpha are such as to suggest strongly that it represents the dismembered distal part of the paroccipito-opisthotic of the Testudinata. This character I pointed out in 1870, and it deserves more attention than it has received from Dr. Baur and other authors. It cannot be seen without taking to pieces the region to which the quadrate is articulated. When this is done it is found that the paroccipital enters as a cone between the exoccipital and petrosal, and extends inwards in Mosasaurus nearly to the region of the semicircular canals. *Nothing like this is to be found in the Lacertilia.* The question now arises, what is the meaning of this structure? As the Pythonomorpha is a cretaceous type, it is evident that it is a survival of some primitive condition, and not a derivative of the condition found in the later order of Lacertilia; where the paroccipital is entirely superficial in its connections. On the contrary, the character of the Lacertilia has been more probably derived from that of the Pythonomorpha by the loss of the proximal part of the paroccipital.

In the Testudinata the paroccipito-opisthotic has not been observed, according to Baur, to consist of two elements distinct at some stage of

embryonic life. This fact does not, however, preclude the possibility that such a division may not have existed among the ancestors of the Testudinata. As this order is very old, these ancestors can only be looked for in the Permian and Triassic periods. Characters which belong to early geologic time, are frequently dropped out of the embryonic record. Now in the Permian Reptilia, some of which are the ancestors of the Testudinata, the quadrate is a short element, and is separated from the exoccipital and the opisthotic by a separate bone which has been called mastoid and mastotympanic by Owen,[3] and which I have considered as part of the "squamosal" in the absence of suture separating it from that element.[4] I think that such an element exists in the Cotylosauria. The periotic bones in Empedias[5] and Chilonyx are far removed from the elements which serve as suspensors of the quadrate bone, and are distinct from them in Chilonyx at least. Owen (l. c.) thinks that a paroccipital has been fused with the exoccipitals in Ptychosiagon (l. c.), and in a position which shows that it could not have been the opisthotic. The homologizing of one or the other of these elements with the paroccipital of the Pythonomorpha is too clearly among the possibilities to be negatived by any evidence to the contrary yet brought forward by Dr. Baur. In fact the origin of the opisthotic element as an ossification about the posterior semicircular canal, renders it a priori probable that an osseous body at a distance from that center, such as the distal part of the paroccipito-opisthotic bones in the Testudinata, was originally distinct, and for this element the name paroccipital is appropriate.

2. *The Exoccipital and Quadrate.*—Dr. Baur again denies that the exoccipital articulates with the quadrate in certain genera of Iguanidæ and gives some figures of that region in the *Conolophus subcristatus* to sustain his allegation. Unfortunately, though he seems to have taken the elements apart, as I suggested that he do, he did not put them together in their original relation when he had them drawn. I now give two drawings traced from the plate of the skull of the same species given by Dr. Steindachner.[6] As these plates represent exactly the characters which I have observed and described in allied genera, I regard them as correct. It will be observed that there is a considerable contact between the exoccipital and the quadrate. There is also con-

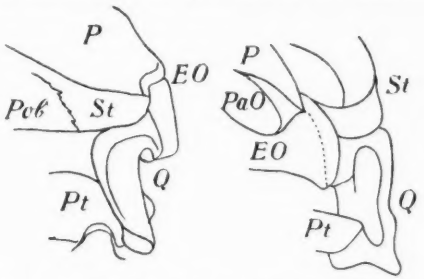
[3] Proceeds. Geolog. Soc. London, 1859, p. 50.

[4] Proceeds. Amer. Assoc. Adv. Sci., 1871, p. 207.

[5] Proceeds. Amer. Philos. Soc., 1885, p. 236; 1895, pl. VIII, fig. 4, otic region of Chilonyx.

[6] Die Schlangen u. Eidechsen der Galapagos Inseln, K. K. Zoolog. Botan. Gess. Wien, 4to. 1876; pl. V.

tact with the supratemporal, and probably with the paroccipital. *The articulation of the quadrate with the exoccipital is universal in the Iguanidæ.* I will, however, here call Dr. Baur's attention to the fact that I nowhere stated that the quadrate of the Lacertilia does not



Conolophus subcristatus Gray. From Steindachner.

touch the supratemporal or the paroccipital. I have simply asserted that the quadrate in the Lacertilia articulates with the exoccipital, and does not do so in the Ophidia. I will notice later some exceptions to the rule, of which I have since obtained information. The articulation with the supratemporal (prosquamosal, Baur) is naturally not to be mentioned in a diagnosis of the order Lacertilia, as there are numerous genera of that suborder in which that element is wanting.—E. D. Cope.

Boulanger on the Difference between Lacertilia and Ophidia; and on the Apoda.—In a recent note,[7] Dr. Boulenger criticizes my definitions of the above suborders and the Pythonomorpha as published in a late number of this journal.[8] He objects to the definition which I gave the Lacertilia, viz.: that the quadrate bone articulates with the exoccipital; and to that of the Ophidia, which asserts that this articulation does not take place. He suggests that I must have examined very few types of Lacertilia or I would not have made such a statement, and he mentions two or three other elements with which the quadrate bone also articulates. Now I must call Dr. Boulanger's attention to the fact that I nowhere state that the quadrate does not touch the other elements he has referred to, but have, on the contrary, stated that they do so touch. The doctor has apparently not read my article carefully, or has injected into his reading some-

[7] Annals and Magaz., Nat. History, XVI, 1895, p. 367.

[8] American Naturalist, 1895, p. 859.

thing which is not there. What he states in his note that bears on my definition is, that the quadrate in Chlamydosaurus does not articulate with the exoccipital, and he gives a figure to substantiate his opinion. I would have preferred to have seen a figure of this structure with the quadrate in place, but I have been able to confirm the observation by the examination of the skull of another Agamid, *Phrynocephalus olivierii*. Here the quadrate articulates with the paroccipital (supratemporal., Boul., squamosal, Baur), and is in contact with the supratemporal (squamosal, Boul.), and little or not at all with the exoccipital. How far the family of the Agamidæ generally present this structure I am unable to say, as but few skeletons of this family are at my disposal. As regards other families I have examined abundant material, as stated in my last communication (l. c., Nov. p. 1004). In review I may say that it is self evident that in general the distinction that I have drawn betweeen Lacertilia and Ophidia in this respect is valid, (1st) because the supratemporal is frequently absent, and therefore not diagnostic; (2d) because the extremity of the paroccipital is insignificant, and affords insufficient support; (3d) because the paroccipital process of the exoccipital is the only remaining element sufficient for the purpose.

Dr. Boulenger next attacks my definition of the Ophidia, alleging that the quadrate articulates with the petrosal (proötic) or with that element and the exoccipital. Here again I am supposed to have stated that the quadrate does not articulate with the (proötic) petrosal, when in fact, I did not mention that element. As to articulation with the exoccipital, I do not consider that this can be regarded as established until the embryology and paleontology are looked into. Because an element cannot be seen in an adult skull, it does not follow that it does not exist. The paroccipital is present in the Tortricidæ, and it is, so far, only an assumption to suppose that it is not represented in the allied Uropeltidæ, and in the less allied Epanodonta and Catodonta.

The decurvature of the parietals and frontals to the basicranial axis in snakes has been cited since Müller, by Huxley,[9] as peculiar to that order, and I know of no exceptions so far as regards the parietals. The optic foramina in some snakes with large eyes are confluent, as I have long been aware, and this foramen is at the expense of the inferior part of the frontals. This, however, does not produce the character of the Lacertilia, and the definition is not invalidated, as Dr. Boulenger alleges; nor is it by the decurvature of this bone and the parietal in the Amphisbænia, where they *do not reach* the sphenoid. I

[9] Anatomy of Vertebrated Animals, p. 203.

did not "admit," as alleged by Boulenger that these Lacertilia "agree with the Ophidia," as they do not. Dr. Boulenger asks "what," under these circumstances, "remains of Prof. Cope's new definition of the suborders of the Squamata?" From what has preceded it is evident, first, that they are not "new" except as to the exoccipital; and second, that they remain intact, so far as any evidence to the contrary has been produced by Dr. Boulenger, except as to the articulation of the quadrate with the exoccipital in two genera of Agamidæ. And it is not necessary to observe that very few groups so closely allied as the Lacertilia and Ophidia can be defined without exceptions.

If we now look at the definitions given by Dr. Boulenger in his volumes of catalogues of lizards and of snakes in the British Museum, the necessity of something better becomes at once apparent. The Lacertilia are thus defined: "Quadrate bone articulated to the skull; parts of the ali- and orbitosphenoid regions fibrocartilaginous; rami of the mandible united by suture; temporal region without or with only one horizontal bar. Anal cleft transverse. Copulatory organs paired. *Gunther.*"

The definition of the Ophidia (dated 1893) is as follows: "Quadrate bone articulated to the skull; brain capsule entirely osseous; rami of the mandible articulated by ligament. Anal cleft transverse. Copulatory organs present paired. *Gunther.*"

In these definitions the first and last two are identical in both. The presence or absence of a horizontal bar is not definitive, and indeed no reference to it is found in the definition of the Ophidia. The only definitions left are those derived from the mode of union of the symphysis mandibuli, and the ossification of the brain case. The former of these characters is not found in several families of Lacertilia, and the latter is the one which Dr. Boulenger has repudiated in the note which gave origin to this reply. I think my attempts at definition do, in point of precision and application, compare very favorably with those which seem to have satisfied Dr. Boulenger in the work cited. In another publication he gives the characters usually employed, which are much better.

In a recent synopsis of the species of Cæciliidæ,[10] Dr. Boulenger makes some observations on the relations of this family to the rest of the Urodela. He remarks: "If the absence of the limbs and reduction of the tail were the only characteristic of the group, I should, of course, not hesitate to unite the Cæcilians with the Urodeles; but, to say nothing of the scales, the Cæcilian skull presents features which are not shared by any of the tailed Batrachians, and the order can be

[10] Proceeds. Zool. Soc., London, 1895, p. 402.

defined by the cranial characters alone. The resemblance of the larval Ichthyophis to Amphiuma is after all superficial, and although, as I believe, the Apoda and Caudata may have evolved from a common stock, Amphiuma is certainly not the connecting form between the two as Prof. Cope would have it, for we cannot well assume the scales, lost in the Urodeles, to have reappeared in the Cæcilians."

The above discussion is interesting but troublesome, because it requires a reply. In the first place, it ought not to be necessary to remark that the presence or absence of scales in the Batrachia is not an ordinal character. On the page following that from which the above is quoted, Boulenger states that six of the sixteen genera of Apoda (Cæciliidæ) *have no scales.* Further, among the extinct Stegocephalia some genera have scales and some have none, so there is reason to suppose that scales may be secondary as well as primary. Moreover, if a a genus of salamanders should be discovered which possesses scales, no one would think of removing it from the Urodela on that account. There is no improbability in the supposition that such a genus may not be found in some of the Mesozoic formations. Second, Prof. Cope has never stated that the genus Amphiuma is the connecting form between the Apoda and Caudata. He has said that the Amphiumoidea probably are, and possibly the Amphiumidæ, but the genus Amphiuma never.[1] He has very rarely alleged that any genus is ancestral to any other genus. There can be no one genus between these two groups, for there is room for several genera. And one may agree with Dr. Boulenger that the Apoda and Caudata have had a common ancestor, and not disagree with the position that the Apoda belong to the Caudata, for there is no reason why that common ancestor may not probably have been one of the Caudata, unless there is more difference in the cranial characters of the two than has been yet pointed out.—E. D. Cope.

ENTOMOLOGY.[2]

Heterocera of the Lesser Antilles.—Reporting on a collection of Geometridæ and allied families from the islands of Grenada, St. Vincent and the Grenadines, Mr. G. F. Hampson[3] says. The Geometridæ

[1] Batrachia of N. America, 1889, pp. 34–222.

[2] Edited by Clarence M. Weed, Durham, N. H.

[3] Ann. and Mag. Nat. Hist., XVI, 329.

are represented by very few species in the Lesser Antilles compared with the large number that exist in other parts of the Neotropical Region both north and south of the isthmus; and almost all the species are identical with those found on the mainland.

The Pyralidæ are represented by a much greater diversity of species; but these, as in other parts of the world, are very wide ranging, most of the species being also found in Brazil and Venezuela, some being identical with forms found in the United States, whilst others range down to Chili; others again being spread throughout nearly the whole tropical zone; whilst, even of the species described as new, several are represented in the British Museum or other collections by specimens from continental localities.

Bot Flies of the Horse.—Prof. H. Garman publishes[3] an interesting account of the habits of oviposition of *Gastrophilus nasalis* and *G. equi*. He enumerates five species of bot flies attacking the horse in America; the adults may be distinguished by the following key:

1 (6) Discoidal cell closed by a cross vein.
2 (3) Wings marked with brown . . . . . *G. equi.*
3 (2) Wings not marked with brown.
4 (5) Anterior basal cell nearly or quite equal to the discoidal cell in length . . . . . *G. nasalis.*
5 (4) Anterior basal cell markedly shorter than the discoidal cell [*G. hæmorrhoidalis.*
6 (1) Discoidal cell not closed . . . . . *G. pecorum.*

Concerning the habits and life history of *G. equi*, the most abundant species, Professor Garman writes:

This fly buzzes about horses during the hot summer days, occasionally alighting on their bodies, and when an opportunity offers, placing its eggs in the hairs on the inside of the knee, on the shoulders, and sometimes even on the mane. Its mouth-parts are in a rudimentary condition, and it can not, even if it were disposed to, do any injury to horses.

It is probable that the grubs recently hatched from the eggs of this fly are taken into the mouths of horses on the lips or tongue. I am told by a gentleman who has had much experience with horses that he has on many occasions taken the eggs between the moistened palms of his hands, and in a few moments felt the young grubs wriggling about. It appears that moisture accelerates the hatching of the eggs, and it is just possible that many eggs would never hatch at all if the eggshell

[3] 7th Rept. Kentucky, Agr. Exp. Station.

was not moistened in some way. Whether this must be from the horse's tongue or lips in all cases is a question which may be considered not yet settled. Professor H. Osborn, of Iowa, is disposed to believe that the young do not hatch unless moistened by the horse's tongue; that the young grubs generally die in the eggs if left for 35 to 40 days; and that they are not commonly ready to hatch until from 10 to 12 days after the eggs are laid.

Fossil Butterflies.—Fossil butterflies are the greatest of rarities. They occur only in tertiary deposits, and out of the myriads of objects that have been exhumed from these beds in Europe and America less than twenty specimens have been found. The great body of these deposits is of course of marine origin, but at least thirty thousand specimens of insects have been recovered from those beds which are not marine. Over fifty thousand insects from the one small ancient lake of Florissant, high up in the Colorado Parks have passed through my hands, yet I have seen from them but eight butterflies. Each of these belongs to a genus distinct from the others, as is also the case with all or all but one, of the butterflies found at Radoboj, at Aix, and at Rott in the European tertiaries. With two (European) exceptions, each represents an extinct genus, and these two exceptions, Eugonia and Pontia, are genera found to-day both in Europe and America. The species, however, are all extinct.

One would hardly expect that creatures so delicate as butterflies could be preserved in a recognizable state in deposits of hardened mud and clay. Yet not only is this the case, but they are generally preserved in such fair condition that the course of the nervures and the color patterns of the wings can be determined, and even, in one case, the scales may be studied. As a rule they are so well preserved that we may feel nearly as confident concerning their affinities with those now living as if we had pinned specimens to examine; and generally speaking the older they are the better they are preserved.—*S. H. Scudder in Frail Children of the Air.*

Origin of European Butterflies.—Mr. W. H. Bath in discussing[4] the effects produced by the glacial period upon the distribution and diversity of European butterflies says: As the result of his investigations Ernest Hoffmann asserts that of the 290 species of Rhopalocera inhabiting our continent at the present time, no less than 173 were originally derived from Siberia. If this was the case, and it seems very

[4] The Entomologist, XXVIII, 247.

likely to be correct, the majority of them probably immigrated westwards of the commencement of the pleistocene periods, for they must be of great antiquity; moreover it is unreasonable to suppose that many of the species could have existed also in the south of Europe, even at the climax of the glacial period. According to the same authority only 8 species have been derived from Africa, and 39 from Asia south of Siberia. These must have immigrated into the south European province of the palearctic region after the termination of the glacial period as they belong to genera and types of tropical distribution. At the present day they occur in those countries bordering on the Mediterranean Sea.

The glacial species of butterflies—that is the most ancient forms, designated by Weismann "the original stirps"—are in many cases distinguished by their melanic and melanochroic tendencies. We thus find the forms inhabiting the more northern localities and the higher elevations on the mountains often of a darker hue, while their representatives in more southern latitudes and less elevated altitudes exhibit a brighter coloration.

North American Aphelininæ.—As the first of a technical series of bulletins to be issued by the Division of Entomology of the U. S. Department of Agriculture, Mr. L. O. Howard publishes a Revision of the Aphelininæ of North America. Regarding the biology of the group Mr. Howard writes: The insects of this subfamily are all, so far as we know, parasitic either upon the Coccidæ, Aleyrodidæ, or Aphididæ. They are evidently many brooded, and issue from their hosts indifferently throughout the warmer months of the year, and through the winter on the insectary. With the Aleyrodidæ, Aphididæ, and the Diaspinæ among the Coccidæ, but one specimen apparently issues from a single host. Sufficient observations have not been made upon the early stages of the Aphelininæ. Their larvæ feed both upon the body of the scale insect and upon the eggs. They attack both sexes of the host, issuing when full-grown through circular holes, cut through the body walls, and, in the case of the Diaspinæ through the scale. With the scale insects of the genus Pulvinaria, the aphelinine larvæ live within the body of the female and not in the waxy egg mass which she secretes.

News.—A List of Night-flying Moths from Kentucky, is published by Prof. H. Garman in the 7th Report of the Experiment Station of that State.

An extended account of the life-history of *Phryganidia californica* Packard is published by Messrs. V. L. Kellogg and T. J. Jack in the Proceedings California Academy of Sciences (Ser. 2, V, 562–570.)

Prof. J. B. Smith issues as Bulletin 111 of the New Jersey Experiment Station an account of experiments with "Raupenlime" and "Dendrolene," substances useful for applying to tree trunks to keep out borers.

PSYCHOLOGY.[1]

American Psychological Association.—The American Psychological Association held its annual meeting this year at the University of Pennsylvania, in connection with the meetings of the scientific societies affiliated with the American Society of Naturalists. Hitherto the Psychological Association has met independently, but the feeling has been growing that the close relation between the more recent forms of psychology and the biological sciences made it eminently suitable and desirable that their representatives should be brought together. The success which has attended this first step makes it probable that the policy will be continued in future.

No official outline of the proceedings of the Psychological Associaciation is at hand, and any account written from memory will be more or less defective. Consequently the present writer must beg indulgence from those whose words he endeavors to report if he has, in any case, misrepresented them. On the whole, however, he believes he is giving a fair outline of the more important points.

At the first session, on Friday, Dec. 27th, the opening paper, on "Physiology and Psychology," was read by Prof. George S. Fullerton of the University of Pennsylvania. Two years ago, at the New York meeting of the Association, Prof. Fullerton outlined the relation in which psychology as a natural science stands to metaphysic, and concluded that psychology should adopt, as far as possible, the methods and assumptions of the other natural sciences, and should relegate the task of criticising those assumptions to a distiuct science—that of metaphysic. The paper read this year was a continuation of the same general line of thought in the investigation of the relations of psychology and physiology. Taking Foster's "Physiology" as a standard, we find, said Prof. Fullerton, that the author is absolutely unable to give any

[1] This department is edited by Dr. Wm. Romaine Newbold, University of Pennsylvania.

account of the functioning of the higher nervous centres without having recourse to sensations, ideas, volitions—in a word, without entering the field that properly belongs to psychology. While it may be not only right, but also necessary, for the physiologist to do this, we must not close our eyes to the fact that the mere fact of its necessity proves the imperfect condition of physiology, and tends to obscure the line dividing physiology from psychology. Prof. Fullerton claimed that the methods employed by the two sciences are distinct, and that it is important to the advancement of knowledge to recognize this distinction.

Dr. Livingston Farrand, of Columbia, submitted a scheme of physical and mental tests which will be used with the students of Columbia to determine, as far as can be done by direct experiment, their capacities in both respects at various stages of their college life. After some discussion, a motion was passed that the President be requested to appoint a committee of five to report upon the advisability of the universities represented taking concerted action in the adoption of some similar scheme.

Dr. Arthur MacDonald, of Washington, D. C., read a paper on "Some Psycho-Neural Data." He reported experiments somewhat similar to those of Dr. Farrand, made upon certain groups in the community, and apparently showing that between definite classes definite physical and mental differences are experimentally discoverable.

Prof. Lightner Witmer, of the University of Pennsylvania, introduced one of his graduate students, Mr. Oliver Cornman, who reported the results of "An Experimental Investigations of the Processes of Ideation." Mr. Cornman's method was that of giving a large number of individuals, usually children, a definite suggestion and requiring them to write for a definite period of time—usually 15 minutes—all the thoughts directly or indirectly suggested by it; he had found that in most of his subjects the idea trains were, for a short time, largely controlled by the concomitant suggestions of the time and place, and consequently the earlier terms of each series showed a marked similarity. This soon disappeared, and the further development of the idea trains seemed dependent upon the character and previous experience of the individual. We have, therefore, in this, a convenient method of "tapping," as it were, the ideational content of the individual. Mr. Cornman pointed out further, that, to get results at all comparable with one another in the case of different bodies of subjects, the original suggestions must be given in identically the same words without explanations or further suggestions on the part of the experimenter, and, to secure this end, should always be written.

At the afternoon session on Friday, Prof. J. McK. Cattell, of Columbia, read his President's Address. It was, on the whole, a defense of that experimental method of which he is the leading representative in this country, and was, therefore, in a way, a reply to the rather unfavorable estimate of the method and its results which had been expressed by Prof. James of Harvard in his President's Address of the preceding year. The burden of Prof. Cattell's argument was found in the statement, that every science is either genetic or quantitative in its method; that those sciences which have been predominently quantitative will undoubtedly, in time, be formulated in genetic terms, that, conversely, into the genetic sciences also, such as biology and psychology, the quantitative method will ultimately be introduced. This is the aim of experimental psychology in the narrower sense. While expressing the strongest conviction of the importance of this experimental method to the science of psychology, Prof. Cattell displayed such moderation in his estimate of the results thus far achieved by it, and such sympathetic insight into the aims and relative values of other methods, that his address was received with the warmest applause by all, and no one could be found to pass a criticism upon it.

Prof. Chas. A. Strong, of the University of Chicago, read a paper on "Consciousness and Time," of which, on account of its exceedingly abstract character, I could not venture to give an analysis from memory.

The morning of Saturday, December 28th, was occupied by a discussion on "Consciousness and Evolution."

Prof. William James, of Harvard, opened the discussion by outlining the general features of the problem at issue: First, whether consciousness is coextensive with the universe or originated in time; second, whether consciousness is an active force capable of controlling brain movement, or whether it is a mere epiphenomenon, produced by the brain but not capable of affecting the brain; third, whether consciousness has been a factor in the production of adaptation.

Prof. Cope, of the University of Pennsylvania, who had been especially requested to take the leading part in the discussion, attacked the question from the point of view of the paleontologist. He held that natural selection is not sufficient to account for adaptation, that the adaptation of the individual organ is the result of use, and that the effects of use can be inherited. In supporting this position he gave many illustrations, based upon his personal observation. He held further that organic evolution involved combinations and recombinations of matter which not only never could have been produced by the opera-

tion of known physical and chemical forces, but were of a character precisely the opposite of their known effects. To account for this, he thought we must assume in organic matter the existence of an activity distinct from all the other activities of nature. Progressive evolution is the chief outcome of this activity, and therefore he had proposed to term it an anagenetic, or upbuilding activity, as opposed to the katagenetic or destructive activities of physics and chemistry. This anagenetic activity Prof. Cope was inclined to believe due to the presence of sensation, and therefore maintained that consciousness is an active factor in the individual and in evolution.

Prof. Cope was followed by Prof. J. Mark Baldwin, of Princeton, who commented upon several points of Prof. Cope's argument, drawing special attention to the fact that recent investigation into the effect on young children of their surroundings makes it more easy to account for adaptation without reference to inheritance of acquired aptitudes. He also deplored the sharp antithesis between the doctrine of consciousness as a cause and as a epiphenomenon, holding that both views found their reconciliation in monism.

Prof. C. Sedgwick Minot, of Harvard, attacked the neo-Lamarckian doctrine from the neo-Darwinian point of view, supporting his position by evidence drawn from his own work in embryology. He suggested, as a speculation, that consciousness, although not itself a force, might be conceived to possess the property of selecting out of the brain forces that one which it is control conduct.

Prof. G. S. Ladd, of Yale, welcomed Prof. Cope's address as an important contribution from the purely scientific point of view to the support of doctrines held by himself in common with many other metaphysicians, and made a plea for the recognition of the metaphysician on the part of scientists as a coworker in the field of knowledge.

Prof. Fullerton, of the University, called attention to our actual ignorance on all these points, and expressed the opinion that fundamental differences exist which cannot be glossed over by such vague doctrines as that of monism.

Other speakers were: Prof. J. H. Hyslop, of Columbia; Dr. D. S. Miller, of Bryn Mawr, and Dr. Wesley Mills, of McGill University, Montreal.

Prof. Cope then concluded the discussion by adducing a series of arguments in favor of the inheritance of acquired attributes, any one of which, he held, would be sufficient to set the matter at rest.

At the afternoon session, Prof. G. T. W. Patrick, of the University of Iowa, reported an experiment on the effects of loss of sleep. A patient

had been kept awake for 90 consecutive hours, during which time careful experimental tests were made of his physical and mental condition, and the results were reported in detail. Among the more interesting of these results were, continuous increase in weight, relatively slight loss of muscular strength, the production of visual hallucinations, and the sudden disappearance of all symptoms after only 10½ hours of sleep—about 25 per cent. of that which had been lost.

Prof. Wesley Mills, of McGill University, Montreal, announced his intention of contributing at the next meeting of the Association further researches on the psychic development of young animals and its physical correlations.

Prof. Lightner Witmer, of the University of Pennsylvania, read a paper on "Variations in the Patellar Reflex as an Aid in Mental Analysis. Dr. Witmer described the apparatus and the method used to determine, 1st, The extent of the normal jerk; 2d, the increment due to the synergic activity of the cortical processes concerned in sensation, thoughts, etc. His results he regarded as tentative only; they appeared, however, to show (1) that sensation or thought processes which did not directly tend to produce movement had little effect upon the knee jerk; (2) that all processes which tended to produce muscular contraction in any part of the body tended to increase the knee jerk; (3) that this increase was quite as marked in the case of the *thought* of a movement as in that of the movement itself.

Prof. James H. Hyslop, of Columbia, reported a series of experiments on hallucinations induced by a crystal. He did not attempt to give any explanation of the phenomena, but pointed out that in two cases the phantasms possibly indicated some unknown method of acquiring information.

Prof. W. R. Newbold narrated informally three cases vaguely described as "Dream Reasoning," which had occurred in the experience of two of his colleagues. Dr. W. A. Lamberton, Professor of Greek in the University of Pennsylvania, when a young man, after giving up as insoluble a problem in descriptive geometry upon which he had been working for weeks by the analytical method, awoke one morning several days later to find an hallucinatory figure projected upon a blackboard in his room with all the lines necessary to a geometrical solution of the problem clearly drawn. He has never had any other visual hallucination. Dr. H. V. Hilprecht, Professor of Assyriology in the University of Pennsylvania, some years ago dreamed an interpretation of the name Nebuchadnezzar which has since been universally adopted. At a later period he dreamed that an Assyrian priest

gave him information about some inscribed fragments that had puzzled him which was afterwards confirmed in all points now capable of confirmation. Dr. Newbold offered a psychological explanation of these curious cases.

Prof. G. S. Fullerton, of the University of Pennsylvania, was elected President, and Dr. Livingston Farrand, of Columbia, Secretary, for the ensuing year.

Among the members present, besides those already mentioned, were Mr. Henry Rutgers Marshall, of New York; Prof. N. S. Gardiner, of Smith College; Dr. H. C. Warren, of Princeton; Prof. E. S. Sanford, of Clarke University; Prof. E. H. Griffen, of Johns Hopkins; Prof. J. C. Creighton, of Cornell; Prof. James Seth, of Brown, and Dr. Warner Fite, of Williams' College.—W. R. N.

The Cat's Funeral.—Every one has observed instances of affection between those proverbially hostile animals, the dog and the cat, but a case cited by l'Eleveur merits especial attention. A dog and a cat belonging to the same master were the best friends in the world, and spent their time in frolicking together. One day, while playing as usual, the cat died suddenly, falling at the dog's feet. The latter, at first, did not realize what had happened, but continued his play, pulling, pushing and caressing his companion, but with evident astonishment at her inertness. After some time he appeared to understand the situation, and his grief found vent in prolonged howls. Presently he was seized with the idea of burying the cat. He pulled her into the garden, where he soon dug a hole with his paws, and put in it the body of his former companion. He then refilled the hole with dirt, and, stretching himself out on the grave, resumed his mournful howling. The idea of burying the dead cat was extraordinary. Whence came the thought? Could it be imitation, or, which is a better explanation, did the dog have a vague idea of concealing the event which might possibly be imputed to him. But then it would seem unreasonable for him to call attention to the fact, by installing himself on the grave and howling. However, even human criminals are sometimes equally inconsistent. It is difficult to form an exact idea of what gave rise to the dog's conduct in this case. (Revue Scientific Juillet, 1895).—E. D. C.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

American Academy of Arts and Sciences.—The 11th of December.—The following papers were read: On the temperature of the crust of the earth at great depths. By Messrs. Alexander Agassiz and P. C. F. West. Palestine in the fifteenth century B. C. according to recent discoveries. By Professor Crawford H. Troy.

Boston Society of Natural History.—December 4th.—The following paper was read: Mr. L. S. Griswold, "The San Francisco Mountains and the Grand Canyon."

December 18th.—The following paper was read. Prof. G. Frederick Wright, "The present status of glacial man in America." The subject of Professor Wright's paper was discussed by Prof. F. W. Putman, Prof. H. W. Haynes, and others.

January 1st, 1896.—The following papers were read: Mr. A. W. Grabau, "Lake Bouvé, a glacial lake in the Boston Basin;" Prof. W. O. Crosby, "Glacial lakes in the valleys of the Neponset and Charles Rivers; and the Post-tertiary history of the Nashua Valley.—SAMUEL HENSHAW, *Secretary.*

January 15th.—The following paper was read: Mr. William Brewster, Notes on the Natural History of Trinidad. Stereopticon views were shown.—SAMUEL HENSHAW, *Secretary.*

New York Academy of Sciences, Section of Biology.—December 9, 1895.—The following papers were presented: Prof. C. L. Bristol, "The Classification of Nephelis in the United States." The study of abundant material, collected from Maine to South Dakota, has shown that the color characters cannot be depended upon for specific determination. An examination of the metameral relations of this leech indicate that not more than a single species occurs in this country. Prof. F. H. Osborn, "Titanotheres of the American Museum of Natural History." The complete skeleton of *Titanotherium robustum* is remarkable in possessing but twenty dorso-lumbar vertebræ, a number identical with that typical of the Artiodactyla, but entirely unique among Perissodactyla. It is now appears probable that the development of horns in the Titanotheres became a purely sexual character, and that the genera Titanops, Marsh and Brontops, Marsh, are founded respectively upon male and female individuals of *Titanotherium robustum.* Dr. J. L. Wortman, "The expedition of 1895 of the Amer-

ican Museum of Natural History." The Expedition passed into the Uinta beds of N. E. Utah, then between the Eastern escarpment of the Uinta range and the Green River into the Washakie Beds of S. W. Wyoming, the most important result geologically being that the Brown Park deposit is found to be of much later age then the Uinta.—BASHFORD DEAN, *Recording Secretary.*

American Philosophical Society.—The following communications were read: "The Use of Photography for the Detection of Differences in Chemical Composition, in Age, and in Fluidity of Inks," Prof. S. P. Sharples. "Some Observations on the Forgery of a Mark," and "Detection of a Forgery in the Fraudulent Use of a Signature Stamp," Dr. Persifor Frazer.

Academy of Natural Sciences.—Philadelphia, December 31st. —The following officers were elected: President, Samuel G. Dixon, M. D.; Vice-Presidents, Thomas Meehan, Rev. Henry C. McCook, D. D.; Recording Secretary, Edward J. Nolan, M. D.; Corresponding Secretary, Benjamin Sharp, M. D.; Treasurer, George Vaux, Jr.; Librarian, Edward J. Nolan, M. D.; Curators, Henry A. Pilsbry, Henry C. Chapman, M. D., Arthur Erwin Brown, Samuel G. Dixon, M. D.; Councillors to Serve Three Years, Uselma C. Smith, William Sellers, Charles E. Smith, John Cadwalader; Finance Committee, Charles Morris, Chas. E. Smith, Uselma C. Smith, William Sellers, Charles P. Perot; Council, Isaac J. Wistar.

The American Morphological Society held its annual meeting at the University of Pennsylvania, Dec. 26, 27, and 28, 1895. The stated business of the first session was the Report of the Committee of Affiliation with the American Society of Naturalists. After considering this report the Society voted against affiliation. The following were elected to membership: C. J. Herrick, Denison University, Granville, Ohio; E. G. Conklin, University of Pennsylvania, Philadelphia, Pa.; F. R. Lillie, University of Michigan, Ann Arbor, Mich.; F. C. Kenyon, Clark University, Worcester, Mass.; T. H. Montgomery, Jr., West Chester, Penna.; J. L. Kellogg, Olivet College, Olivet, Mich.; J. I. Peck, Williams College, Williamstown, Mass.; and A. D. Meade, Providence, R. I.

At the second session, December 27, the following papers were read and discussed: "Panplasm," by Prof. C. S. Minot; "The History of the Centrosome in Thalassema," by Mr. B. B. Griffin; "The Centrosome in its Relation to Fixing and Staining Agents," by Prof. E. B. Wilson; The Production of Artificial Archoplasmic Centers," by Prof.

T. H. Morgan; "Cell Size and Body Size," by Prof. E. G. Conklin; "The Development of Isolated Bastomeres of the Egg of Amphioxus," by Prof. T. H. Morgan; and "On the Smallest Part of Stentor Capable of Regeneration," by F. R. Lillie (read by the Secretary). The following officers were elected for the ensuing year: President, Prof E. L. Mark, Harvard University; Vice-President, Prof. H. F. Osborn, Columbia College; Secretary and Treasurer, Dr. G. H. Parker, Harvard University. Members of the Executive Committee elected from the Society at large, Prof. E. G. Conklin, University of Pennsylvania, and Prof. W. Patten, Dartmouth College.

At the third session, December 28, the following papers were read and discussed: "Gastrulation of Teleosts," by Dr. Bashford Dean; "Pigment Changes in the Eye of Palæmonetes," by Dr. G. H. Parker" "Reaction of Metridium to Food and other Substances," by Dr. G. H. Parker; "Some Points in the Anatomy of Anoplocephaline Cestodes," by Dr. C. W. Stiles; and "Development of Cassiopea from Buds," by Dr. R. P. Bigelow. After passing resolutions of thanks to the University of Pennsylvania, the American Philosophical, Society, and the Philadelphia Local Committee, the Society adjourned *sine die.*

The American Society of Naturalists,—Met in the Hall of Department of Arts and Sciences of the University of Pennsylvania, on Thursday December 26th and Friday, December 27th, 1895. *Thursday, Dec. 26th,* 2 *P. M.* I. Reports of Committees. II. Special Reports. III. Recommendation of new members. IV. Address by the President, E. D. Cope. "The Formulation of the Natural Sciences." V. Special Papers, Prof. B. Wilder on the teaching of Comparative Anatomy. 8 *P. M.* Illustrated Lecture at the Hall of the Academy of Natural Sciences, by Professor W. B. Scott, of Princeton University, on "The American Tertiary Lakes and their Mammalian Faunas." 9 *P. M.* Reception to all the Societies given by Professor Horace Jayne, at his house on the S. E. corner of 19th and Chestnut Streets. *Friday, December 27th,* 9 *A. M.* The following new members were elected: Professor C. L. Bristol, Dr. F. C. Kenyon, Dr. W. E. Rotzell, L. O. Howard, Professor John Dewey, G. H. Girtz, Dr. A. D. Mead, Professor G. S. Fullerton, Professor J. McK. Cattell, Professor G. T. Ladd, Reid Hunt, Professor William James, Dr. F. Baker, Dr. G. E. Stone, Professor J. M. Baldwin, Dr. T. S. Palmer, George Lefever,

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C. Bumpus, of Brown University; Treasurer, Prof. John B. Smith, of New Brunswick, N. J.; Executive Committee, Prof. Horace Jayne, of Philadelphia, and Prof. Wm. F. Ganong, of Smith College, Mass.

The following committees were apppointed: On Vivisection; Drs. Patton, Sedgwick and Stiles. On the American table at the Naples Zoological Station; Drs. Conn and Stiles. On Antarctic exploration; Professors Heilprin, Osborn and Goodale. The Society elected Prof. E. D. Cope as its representative on the committee to consult with the American member of the committee of the International Congress of Zoologists on Nomenclature. 10 *A. M.* Discussion. *Subject:* The Origin and Relations of the Floras and Faunas of the Antarctic and Adjacent Regions. Geology. Prof. Angelo Heilprin, Philadelphia Academy Natural Sciences. Paleontology. Prof. W. B. Scott, Princeton University. 2 *P. M.* Continuation of the Discussion. Botany. Prof. N. L. Britton, Columbia College. Zoology. Vertebrata, Dr. Theo. Gill, Smithsonian Institution. 7.30 *P. M.* Annual Dinner of the Affiliated Societies at the Lafayette Hotel, north-west corner of Broad and Sansom streets.

Association of American Anatomists.—This body met in Philadelphia, on Dec. 27th and 28th, at the University of Pennsylvania.—*Friday Morning, December 27th,* 8.30 o'clock.—Meeting of Executive Committee. 9.30 o'clock.—Opening of the session by the President. Report of Secretary and Treasurer. Report of Executive Committee. Report of Delegate to Congress of American Physicians and Surgeons. Report of Committee on Anatomical Nomenclature. Report of Committee on Anatomical Material. Report of Committee on Circular concerning Anatomical Peculiarities of the Negro. Report of Dr. Allen, of the Smithsonian Committee on the Table at Naples. Election of members. Other new business. Reading of Papers and Discussions.—1. "Myology of the Extremities of Lemur bruneus" illustrated by drawings and casts of muscles. Dr. George S. Huntington, N. Y. City; 2. "History of the Ciliary Muscle," Dr. Frank Baker, Washington, D. C.; 3. "Absence of Fibrous Pericardium of left side." Illustrated by specimen, Dr. Addinell Hewson, Philadelphia, Pa. "The Descriptive Anatomy of the Human Heart," Dr. Wm. Keiller, Galveston, Texas. *Friday Afternoon,* 2.30 oclock. —Miscellaneous business. Reading of Papers and Discussions.—5. "Nomenclature of Nerve Cells," Dr. Frank Baker, Washington, D. C.; 6. "The Cerebral Fissures of two Philosophers." Illustrated by specimens and photographs, Dr. B. G. Wilder, Ithaca, N. Y.; 7. "The Human Paroccipital Fissure. Should it be recognized and so Designated." Illustrated by specimens and photographs, Dr.

T. H. Morgan; "Cell Size and Body Size," by Prof. E. G. Conklin; "The Development of Isolated Bastomeres of the Egg of Amphioxus," by Prof. T. H. Morgan; and "On the Smallest Part of Stentor Capable of Regeneration," by F. R. Lillie (read by the Secretary). The following officers were elected for the ensuing year: President, Prof E. L. Mark, Harvard University; Vice-President, Prof. H. F. Osborn, Columbia College; Secretary and Treasurer, Dr. G. H. Parker, Harvard University. Members of the Executive Committee elected from the Society at large, Prof. E. G. Conklin, University of Pennsylvania, and Prof. W. Patten, Dartmouth College.

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Wilder; 8. "Practical Histology for large classes" Dr. Chas. S. Minot, Boston, Mass. In the evening a subscription dinner was given by the members of the affiliated societies at the Hotel Lafayette. *Saturday Morning, December 28th.*—Miscellaneous business of minor importance was transacted and these officers elected: Dr. Frank Baker, of Washington, D. C., President; Dr. B. G. Wilder, of Ithaca, N. Y., First Vice-President; Dr. F. J. Shepherd, of Montreal, Canada, Second Vice-President; Dr. D. S. Lamb, of Washington, D. C., Secretary and Treasurer; Delegate to Congress of American Physicians and Surgeons, Dr. Addinnell Hewson, Philadelphia; Alternate, Dr. D. K. Shute, of Washington, D. C. Reading of Papers and Discussions.—9. "Some novel methods of description of the human skull" Dr. Harrison Allen, Philadelphia, Pa.; 10. "Type forms and nomenclature of mammalian teeth." Illustrated by models and diagrams, Prof. Henry F. Osborn, New York City; 11. "The work of the German Anatomical Society in Nomenclature," Dr. Charles Heitmann, New York City. *Sunday Afternoon*, 2.30 o'clock.—Miscellaneous business. Reading of Papers and Discussions; 12. "*Fossa capitis femoris* with observations on the trochanteric fossa." Illustrated by specimens, Dr. F. J. Brockway, New York City; 13. "Note on the appearance of a unilateral tuberosity in place of the trochanteric fossa." Illustrated by specimen, Dr. D. S. Lamb, Washington, D. C.

The American Physiological Society.—The eighth Annual Meeting of the American Physiological Society was held in Philadelphia on December 27th and 28th, 1895. The meeting was preceded by the usual smoke talk upon the evening of December 26th. Three of the four formal sessions of the Society were held at the University of Pennsylvania, the fourth at the Jefferson Medical College, The following communications were presented and discussed:

R. H. Chittenden, The mucin of the white fibrous connective tissue; A. R. Cushny, The distribution of iron in the Invertebrates; J. J. Abel, A preliminary account of the chemical properties of the pigment of the negro's skin (with W. S. Davis); T. B. Aldrich, On the Chemical and physiological properties of the fluid secreted by the anal glands of Mephitis mephitica; G. Lusk, Phloridzin diabetes and the maximum of sugar from proteid; W. T. Porter, Further researches on the coronary arteries; G. N. Stewart, Note on the quantity of blood in the lesser circulation; C. F. Hodge, Histological characters of lymph as distinguished from protoplasm; C. F. Hodge (for J. R. Slonaker), Demonstration of the comparative anatomy of the fovea centralis; G. C. Huber, The ending of the chorda tympani in the sublingual and the

submaxillary glands (with demonstrations); G. W. Fitz, A working model of the eye; J. G. Curtis, A method of recording muscle curves; G. N. Stewart, Demonstration: Measurement of the circulation time of the retina; T. W. Mills, Cortical cerebral localization in certain animals; W. T. Porter, A new method for the study of the intracardiac pressure curve; S. J. Meltzer, On the mode of absorption from the peritoneal cavity in rabbits, (with I. Adler); S. J. Meltzer, On the incorrectness of the often quoted experiments of Starling and Tubby with reference to the mode of absorption from the peritoneal cavity in dogs; F. S. Locke, Of the action of ether on contracture and of positive kathodic polarisation of voluntary muscle; H. G. Beyer, On the influence of exercise on growth; W. H. Howell (for Messrs. Conant and Clark), The existence of a separate inhibitory and accelerator nerve to the crab's heart; Fr. Pfaff, On Toxicodendral and on the socalled toxicodendric acid; H. C. Chapman, Methods of teaching physiology.

The following persons were elected to membership in the Society:

J. G. Adams, Professor of Pathology, McGill University; T. B. Aldrich, Instructor in Physiological Chemistry, Johns Hopkins University; J. M. K. Cattell, Professor of Experimental Psychology, Columbia College; G. P. Clark, Professor of Physiology, Syracuse University; R. H. Cunningham, Assistant Demonstrator of Physiology, Columbia College; G. W. Fitz, Assistant Professor of Physiology and Hygiene, Harvard University; T. Hough, Assistant Professor of Physiology, Massachusetts Institute of Technology; R. Hunt, Fellow in Physiology, Johns Hopkins University; F. S. Locke, Instructor in Physiology, Harvard Medical School.

Professors C. S. Minot and C. F. Hodge were appointed to express to Professor Langley the opinion of the Society that it is highly desirable that the table of the Smithsonian Institution at the zoological station of Naples be continued. Mr. W. B. Saunders entertained the members of the Society at luncheon at the Art Club. The courtesies that were extended to the affiliated societies by the University of Pennsylvania and the Philadelphia Local Committee were also enjoyed. Officers for the year 1895–96 were elected as follows:

Members of the Council: H. P. Bowditch, R. H. Chittenden, W. H. Howell, F. S. Lee, J. W. Warren; President, R. H. Chittenden; Secretary and Treasurer, F. S. Lee.

The President and the Secretary were appointed respectively delegate and alternate to the Congress of American Physicians and Surgeons of 1897.—Frederic S. Lee, *Secretary.*

The Geological Society of America held its eighth Annual Meeting, and the fifteenth meeting of the Society in the Geological Museum of the University of Pennsylvania, December 26th to 28th. The number of Fellows in attendance was sixty. The first session was convened at 2 o'clock on Thursday afternoon with President N. S. Shaler in the chair. The report of the Council, consisting of the detailed reports of the officers for the year 1895, was submitted in print. This report showed a properous condition of the Society; following are some of the items: membership 226, libraries subscribing for the bulletin 59, receipts during the year from the sale of the bulletin $461.50, number of exchanges 85. The library is deposited with the Case Library at Cleveland. Besides printing six volumes of the bulletin, $3000 has been invested as a publication fund.

Announcement was made of the election by transmitted ballots of officers for 1896 as follows:

President, Joseph LeConte; First Vice-President, C. H. Hitchcock; Second Vice-President, Edward Orton; Secretary H. L. Fairchild; Treasurer, I. C. White; Editor, J. Stanley Brown; Councillors, B. K. Emerson, J. M. Safford.

The following Fellows were declared elected: Harry F. Bain, Des Moines, Iowa; William K. Brooks, Baltimore, Md.; Charles R. Eastman, Cambridge, Mass.; Henry B. Kummel, Trenton, N. J.; William H. Norton, Mt. Vernon, Iowa; Frank B. Taylor, Fort Wayne, Ind.; Jay B. Woodworth, Cambridge, Mass.

A memorial of James D. Dana, written by Joseph LeConte, was read by H. S. Williams. This was not only an appreciative sketch of Dana's life, but an admirable discussion of the true character of geology as a science, and of the great influence of Dana in giving geology a commanding position.

Other short memorials of Henry B. Nason, Albert E. Foote and Antonio del Castillo were read.

A message of regard was voted to J. P. Lesley, who was unable to attend the meeting on account of illness.

The Society held a morning and an afternoon session on Friday and a morning session on Saturday. It was announced that the next summer meeting, to be held in August in connection with the American Association for the Advancement of Science, would be devoted chiefly to excursions.

The list of papers read was not as long as at the Baltimore meeting, but the program was of excellent quality. Following are the titles of the papers presented:

George P. Merrill, Disintegration and decomposition of diabase at Medford, Mass.; Charles R. Keyes, The geographic relations of the granites and porphyries in the eastern part of the Ozarks; J. F. Kemp, Illustrations of the dynamic metamorphism of anorthosites and related rocks in the Adirondacks; N. S. Shaler, The importance of volcanic dust and pumice in marine deposits; L. V. Pirsson, A needed term in petrography; John J. Stevenson, The Cerrillos coal field of New Mexico; N. S. Shaler, The relations of geologic science to education. (Presidential address), ; W. M. Davis, Note on the outline of Cape Cod; W. M. Davis, Plains of Marine and subaërial denudation; F. P. Gulliver, Cuspate forelands; M. R. Campbell, Drainage modifications and their interpretation; N. H. Darton, Some fine examples of stream robbing in the Catskill Mountains; Robert Bell, Proofs of the rising of the land around Hudson Bay; C. R. Van Hise, Movements of rocks under deformation; Alfred C. Lane, Possible depth of mining and boring; Harry Fielding Reid, Notes on glaciers; Frank Leverett, The relation between ice lobes, south from the Wisconsin driftless area; Frank Leverett, The loess of western Illinois and southeastern Iowa; G. Frederick Wright, High level terraces of the middle Ohio and its tributaries; H. L. Fairchild, Four great kame areas of western New York; Warren Upham, Preglacial and postglacial channels of the Cuyahoga and Rocky Rivers; C. H. Hitchcock, Paleozoic terranes in the Connecticut Valley; C. Willard Hayes, The Devonian formations of the southern Appalachians; N. H. Darton, Notes on relations of lower members of costal plain series in South Carolina; N. H. Darton, Resumé of general stratigraphic relations in the Atlantic costal plain from New Jersey to South Carolina; T. C. Chamberlin, The Natchez formations; Arthur Keith, Some stages of Appalachian erosion.

The American Psychological Association met at the University of Pennsylvania, Philadelphia, Friday and Saturday December 27 and 28, 1895.

Friday, December 27, 10 *A. M.*—Psychology and Physiology, Professor George S. Fullerton; Description of a Series of Physical and Mental Tests on the Students of Columbia College, Dr. Livington Farrand; Some Psycho-Neural Data, Dr. Arthur MacDonald; An Experimental Investigation of the Processes of Ideation, Mr. Oliver Cornman. (Introduced by Professor Lightner Witmer).

2.30 *P. M.*—Address of the President, Professor J. McKeen Cattell; Consciousness and Time, Professor Charles A. Strong; Some Conditions of Will Development, Brother Chrysostom; A Psychological Interpre-

tation of the Rules of Definition in Logic, Professor Alfred H. Lloyd.

Saturday, December 28, 10 *A. M.*—Discussion on Consciousness and Evolution, Professors William James, E. D. Cope, J. Mark Baldwin and G. S. Ladd.

2.30 *P. M.*—An Experiment on the Effects of Loss of Sleep, Professor G. T. W. Patrick; Further Researches on the Psychic Development of Young Animals, and its Physical Correlation, Professor Wesley Mills; Variations in the Patellar Reflex as an Aid in Mental Analysis, Professor Lightner Witmer; Experiments on Induced Hallucinations, Professor James H. Hyslop; A Case of Dream Reasoning, Professor W. Romaine Newbold.

Informal communications were made at various times during the sessions.

A fuller account of the papers and discussions will be found in our department of Psychology; q. v.

Indiana Academy of Science.—The eleventh Annual Meeting of the Indiana Academy of Science was held at Indianapolis, December 27th and 28th.

The session was of unusual interest and the attendance good. Forty two new members were elected. This indicates the interest that is being aroused in the State in scientific lines.

The address of the retiring President, A. W. Butler, of Brookville, on "Indiana: A Century of Changes in the Aspects of Nature," met with enthusiastic applause.

A poem on the "Naturalist" recited by W. W. Pfrimmer was a novel, yet enjoyable feature.

The report of the biological Survey of Turkey Lake was another new feature of the meeting, and attracted much favorable attention.

The following papers were presented:

Unconscious Mental Cerebration, C. E. Newlin; Human Physiology in its Relation to Biology, Guido Bell; A means of preventing Hog Cholera, D. W. Dennis; The Hopkins Seaside Laboratory at Pacific Grove, Cal., B. M. Davis; Glacial and Eolian Sands of the Iroquois and Tippecanoe River Valleys, A. H. Purdue; The recent earthquakes east of the Rocky Mountains, A. H. Purdue; Some minor processes of Erosion, J. T. Scoville; Kettle Holes at Maxinkuckee, J. T. Scoville; Fossils from sewer trenches in the Glacial Drift, Wm. M. Whitten; Relief map of Arkansas, John F. Newsom; Notes on the Fauna of the black shales of Bartholomew and Jackson Counties, V. F. Marsters; Botanical Literature of the State Library, John S. Wright; Microscope slides of vegetable material for use in Determinative work, John

S. Wright; Embryology of *Hydrastis canadensis*, Geo. W. Martin; Some determinative factors underlying Plant Variation, Geo. W. Martin; Variations in the cleavage of the *Fundulus* Egg, Geo. W. Martin; Hæmoglobin and its Derivatives, A. J. Bigney; Effects of heat upon the Irritability of Muscle, A. J. Bigney; The evolution of sex in *Cymatogaster*, C. H. Eigenmann; The circulation of protoplasm in the manubrium of *Chara fragilis*, D. W. Dennis; A new Subterranean Crustacean from Indiana, W. P. Hay; A peculiar crawfish from southern Indiana, W. P. Hay; A note on the breeding habits of the cave salamander, *Spelerpes maculicaudus*, W. P. Hay; Notes on a collection of fishes from Dubois County, Indiana, W. J. Moenkhaus; The geographical variation of *Etheostoma nigrum* and *E. olmstedi*, W. J. Moenkhaus; A revision and synonomy of the *Parrus* group of *Unionidæ*, with 6 plates, R. Ellsworth Call; The fishes of the Missouri River Basin, B. W. Evermann and J. T. Scoville; Recent investigations concerning the Redfish (*Oncorhynchus nerka*) at its spawning grounds in Idaho, B. W. Evermann and J. T. Scoville; Additional notes on Indiana birds, A. W. Butler; A mammal new to Indiana, A. W. Butler; Some beneficial results from the use of Fungicides as a preventive of Corn Smut, Wm. Stuart; Ratio of alcohol to yeast in Fermentation, Katherine E. Golden; Distribution of *Orchidaceæ* in Indiana, Alida M. Cunnigham; A new station for *Pleodorina*, Severance Burrage; Additional notes on Animal Parasites collected in the State, A. W. Bitting; Report upon certain collections presented to State Biological Survey, Stanley Coulter; Infection by Bread, Katherine E. Golden; Certain plants as an index of Soil Character, Stanley Coulter; Forms of *Xanthium canadense* and *X. strumarium*, J. C. Arthur; A new habitat for *Gastrophilus*, A. W. Bitting; Noteworthy Indiana Phanerogams, Stanley Coulter.

The following reports relating to the State Biological Survey were made:

Second contribution to the knowledge of Indiana Mollusca, R. Ellsworth Call; Contributions to the Biological Survey of Wabash County, Albert B. Ulrey; Report of the Biological Survey, Zoölogy, C. H. Eigenmann.

Turkey Lake has been taken as a station for exhaustive study of a limit of environment and the variation of its inhabitants, and the following reports represent the first seasons work:

First Report of the Biological Station, C. H. Eigenmann; Some of the physical features of Turkey Lake, D. C. Ridgley; Hydrographic map of Turkey Lake, J. Juday; Temperatures of Turkey Lake, J. P.

Dolan; Inhabitants of Turkey Lake in general, C. H. Eigenmann; *Hirudinea* of Turkey Lake, Bessie C. Ridgley; *Rotifera* of Turkey Lake, D. C. Kellicott; *Clodocera* of Turkey Lake, E. S. Birge; *Mollusca* of Turkey Lake, R. Ellsworth Call; *Odonata* of Turkey Lake, D. C. Kellicott; Fishes and tailed batrachians of Turkey Lake, C. H. Eigenmann; Tailless batrachians of Turkey Lake, C. Atkinson; Snakes of Turkey Lake, H. G. Reddick; Turtles of Turkey Lake, C. H. Eigenmann; Water birds of Turkey Lake, N. M. Chamberlain; Flora of Turkey Lake, O. H. Meincke; Methods of determining Variations, C. H. Eigenmann; Variation of *Etheostoma* of Turkey and Tippecanoe Lakes, W. J. Moenkhaus.

The officers for the next year are as follows:

President, Stanley Coulter of Purdue University; Vice-President, Thos. C. Gray of Rose Polytechnic; Secretary, John S. Wright of Indianapolis; Assistant Secretary, A. J. Bigney of Moores Hill College; Treasurer, W. P. Shannon of Greensburg.

A. J. BIGNEY, *Assistant Secretary.*

The Biological Society of Washington.—November 30th, the following communications were read: Edw. L. Greene, Some Fundamentals of Nomenclature; Theo. Holm, Contributions to the flora of the District of Columbia; David White, The Mode of Development of Exogenous Structure in Paleozoic Lycopods, a review of Williamson and Renault.

SCIENTIFIC NEWS.

Notice Concerning the Geological Map of Europe, Published Under the Auspices of the International Congress of Geologists.—At the Third Session of the International Congress of Geologists, held in Berlin in 1885, the committee on a geological map of Europe made a report, in which the following conditions of publication were announced (Berlin Volume, page LXII): "The house of Reimer & Co., undertakes the publication at its own expense on the sole condition that the international committee guarantee the sale of 900 copies at 100 francs per copy, and furnishes the sum in advance.

The subscription price of 100 francs will be augmented to 125 francs in the regular book trade.

The committee has divided this guarantee subscription as follows: Each one of the large countries of Europe (to wit: Great Britain, France, Spain, Italy, Austro-Hungary, Germany, Scandinavia and Russia) agrees to take 100 copies. The six small countries (i. e., Belgium, Holland, Denmark, Switzerland, Portugal, Roumania) will divide among them the remaining 100 copies, etc."

In the Fourth Session of the Congress held in London in 1888, the following note occurs in the report of the proceedings of the committee on the geological map of Europe (London Volume, p. 59).

"The American committee requested of the Directory to be admitted as a subscriber to the map of Europe on the same terms as the great countries of Europe ('sic') i. e.. for at least one hundred copies and at the same price."

Dr. Frazer, the Secretary of the American Committee, obtained the names of American subscribers to the "one hundred copies at the same price" (100 francs), within a short time of the granting of this request, and promptly notified the publication committee in Berlin, Messrs. Beyrich and Hauchecorne, of the fact.

It appears, however, the map is being offered for sale in the German catalogues at the price mentioned in the Berlin resolution as that accorded to original subscribers.

On this account the undersigned advises the survivors of those who so patriotically came forward in 1888 to enable the geologists of the United States to enjoy same privileges as those of the great countries of Europe, to send through their own agents for the geological map of Europe, since there would no longer be any advantage in obtaining them through a single channel.

List of subscribers to the geological map of Europe in the order of their subscriptions, with number of copies:

Williams College, 1; Ohio State Univ., Columbus, 1; Rensselaer Polytechnic Institute, 1; University of Virginia, 1; Am. Inst. of Mining Engineers, 1; Amherst College, 1; Cornell University Library, 1; Provincial Museum, Halifax, 1; Wesleyan University, Middletown, Conn., 1; Lehigh University, Bethlehem, Pa., 1; Academy of Natural Sciences, Philadelphia, 1; Univ. of California, Berkely, Cal., 1; Prof. C. H. Hitchcock, for Dartmouth College, 1; Prof. J. S. Newberry (dead), 1; Indiana University, 1; Smith College, Northampton, Mass., 1; U. S. Geological Survey, Washington, D. C., 3; Rutgers College, New Brunswick, N. J., 1; Yale University Library, 1; American Geographical Society, 1; Peter Redpath Museum, McGill College, Montreal, 1; U. S. Military Academy, West Point, N. Y., 1; Prof. G.

A. König, 1; N. Y. State Library, Capitol, Albany, 2; Eckley B. Coxe, Drifton, Pa. (dead), 2; University of Nebraska, 1; Kansas State Library, 1; B. S. Lyman, 1; Johns Hopkins University, 1; F. W. Matthieson, La Salle, Ill., 1; Lehigh Valley R. R. Co., Philadelphia, 1; E. V. d'Invilliers, Philadelphia, 1; University of Wisconsin, Madison, Wis., 1; Second Geological Survey of Pennsylvania, 2; State Mining Bureau of California, 1; Washington University, 1; Dr. R. W. Raymond, 1; Franklin Institute, Phila., 1; Harvard College Library, 1; University of North Carolina, Chapel Hill, 1; University of the City of New York, 1; Massachusetts Agric. College, Amherst, 1; W. S. Keyes, San Francisco, Cal., 1; R. D. Baker, Philadelphia, 2; S. F. Emmons, U. S. Geological Survey, Washington, D. C., 1; H. M. Sims, Shenandoah, Page Co., Va., 1; American Museum of Natural History, N. Y., 1; Prof. Alexander Winchell, Univ. of Mich., Ann Arbor (dead), 1; H. Huber, Argentine, Kansas, 1; Jas. E. Mills, E. Quincy, Cal., 1; Cooper Union, N. Y., 1; Collegiate and Polytechnic Institute, Brooklyn, 1; Cornell University, N. Y., 1; Joseph D. Potts, Philadelphia (dead), 1; Prof. J. C. Fales, Danville, Ky., Centre College, 1; T. H. Aldrich, Blocton, Ala., 1; Chas. Paine, Pittsburg, 1; Colorado School of Mines, Golden, Col., 1; Western Reserve Univ. (d. E. W. Morley), Cleveland, Ohio, 1; F. Klepotoko, Houghton, Michigan, 1; Thos. Macfarlane, Ottawa, Canada, 1; Arkansas Geological Survey, Little Rock, 1; Buchtel College, Akron, Ohio, 1; Mercantile Library, Philadelphia, 1; University of Michigan, Ann Arbor, 1; Alabama Geological Survey, University of Alabama, 1; E. S. Whelen, Philadelphia (dead), 1; Worcester Polytechnic Institute, 1; Julius Bien, N. Y., 1; W. A. Ingham, 1; Dr. Jas. P. Kimball, 109 East 15th St., N. Y. City, 1; Dr. J. S. Newberry, N. Y., Dec. 29, '87, 1; New Harmony Institution, Ind., 1; R. Ellsworth Call, Des Moines, Iowa, 1; Bost. Soc. Nat. Hist., 1; Hastings, Jno. B., Ketchum, Alturas Co., Idaho, 1; Geol. Surv. of Minn., Minneapolis, Minn., 1; Lacoe, R. D., Pittston, Luzerne Co., Penna., 1; Vassar College, Poughkeepsie, N. Y., 1; Mt. Holyoke Seminary, South Hadley, Mass., 1; Colby University, Waterville, Me., 1; Cincinnati Soc. of Nat. History, 1; Packer Collegiate Institution, Brooklyn, N. Y., 1; Emmens, Stephen H., Harrison, N. Y., 1; School of Mines, Rapid City, Dakota Territory, 1; Ohio University, Athens, O., Prof. A. D. Morrill, 1; Proctor, John R., Franklin, Ky., Aug. 19, '88, 1; Rose Polytechnic School, Terre Haute, Ind., Aug. 19, '88, 1; Read, Jas. P.. Calico, San Bernard Co., Aug. 31, '88, 1; Oberlin College, Ohio, Aug. 23, '88, 1; Frazer, Persifor, Philadelphia, 1; Streator Township High School, La Salle Co., Ill., R. Wil-

liam Brice, Sept. 21, '88, 1; State Univ., Athens, Ga., Prof. J. W. Spencer, Nov. 12, 1888, 1; Lowry, Thos., Minneapolis, Minn., Nov. 13, 1888 (N. H. Winchell), Nov. 13, 1.—Total, 100.

Dr. Eugenio Dugès died in Morelia, Mex., Jany. 13th. 1895. He was born in Montpellier, France, but had resided in Mexico since 1865. He was a special student of Coleoptera, and had furnished students in the United States with many specimens.

Dr. Adolf Gerstäcker, Professor of Zoology in the University of Griefswald, died June 20th, 1895. He was born Aug. 30th, 1828, and is widest known from his share in the Zoologie of Carus and Gerstäcker and his contributions to Bronns' Thierleben.

Dr. Th. Ebert has been called as Professor of Paleontology to the Prussian Geological Institute, and Dr. Müller as Professor of Regional Geology in the same institute.

Henry John Carter, well known for his researches on Protozoa, Sponges, etc., died at Rudleigh Salterton, England, May 4th, 1895.

Dr. Wm. H. Flower, of the British Museum, has been elected corresponding member for anatomy of the Paris Academy of Sciences.

Dr. W. I. Nickerson, of the University of Colorado, has been appointed Instructor in Biology in the University of Evanston, Ill.

Prof. A. Sabatier, of Montpellier, has been elected corresponding member for Zoology of the Paris Academy of Sciences.

Dr. F. Schütt, of Kiel, has been appointed Professor of Botany and Director of the Botanical Gardens at Griefswald.

Dr. Joseph G. Norwood, the well-known geologist and paleontologist, died at Columbia, Mo., May 6th, 1895.

Dr. E. Hering, of Prague, becomes Professor of Physiology at Leipzig, as successor to the late Prof. Ludwig.

Dr. René duBois Raymond is assistant in the experimental division of the Physiological Institute in Berlin.

Dr. W. A. Setchell, of Yale College, has been appointed Professor of Botany in the University of California.

Dr. F. Sansoni, Professor of Mineralogy in Pavia, and editor of the Italian Journal of Mineralogy, is dead.

Mr. Charles D. Aldright has been appointed Instructor in Biology at the University of Cincinnati.

C. C. Babington, Professor of Botany in the University of Cambridge, died July 22d, aged 86.

James Mortimer Adye, an entomologist, died at Bournemouth, England, May 30th, 1895, aged 34.

Dr. Jas. E. Humphrey has been appointed Lecturer in Botany in Johns Hopkins University.

Dr. A. Kowalevsky has been elected a foreign associate of the Academy of Sciences of Paris.

Prof. J. G. Agardh has given his magnificent collection of Algæ to the University of Lund.

Pietro Doderlein, Professor of Zoology and Geology in Palermo, died March 28, aged 84.

Dr. Pellegrino Strobel, geologist and Conchologist, died at Parma, Italy, June 9th, 1895.

Dr. R. Hanitsch has gone as Director to the Raffles Museum and Library at Singapore.

The Linnean Society of London has awarded a gold medal to Prof. F. Cohn, of Breslau.

Dr. Gustav von Nordenskiold, ethnographer and crystallographer, of Stockholm, is dead.

Dr. A. D. Mead has been appointed Instructor in Neurology in Brown University.

Dr. Reinitzer, of Prag, has been called as Extraordinarius Professor of Botany to Graz.

Dr. E. Schöbl succeeded Dr. Schiemenz as Librarian of the Naples Zoological Station.

Prof. E. D. Cope has been elected associate member of the Academy of Sciences, Arts and Letters of Belgium.

Dr. R. Bonnet, of Giessen, goes as Professor Ordinarius of Anatomy to Griefswald.

Dr. W. Roux, of Innspruck, has gone as Ordinary Professor of Anatomy to Halle.

Dr. Hans Schinz is appointed Ordinary Professor of Botany in Zürich.

Julien Deby, of London, microscopist and student of diatoms, is dead.

Dr. F. C. Kenyon has gone to Clark University as Fellow in Biology.

Dr. H. Lenk, of Leipzig, has made Extraordinarius of Geology.